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RECENT EXPLORATIONS IN NORTHWESTERN TEXAS

BY WARREN K. MOOREHEAD

SOME years ago specimens were sent me from the Canadian valley in Texas. These seemed to indicate something different from the ordinary artifacts from the Southwest. As a result of the acquisition of these few objects two expeditions were sent up the Arkansas valley. The first passed through Oklahoma and Kansas, the local work in Oklahoma being left in charge of Professor J. B. Thoburn of the Oklahoma State Historical Society. This survey continued on up the main river to its source in Colorado. It crossed to the Canadian just over the New Mexico line, came down to Handley's ruins, and remained there three weeks, ending its season at Muskogee in November, 1917. Some thirty sites were found and mapped. In June, 1919, C. B. Franklin, Esq., was sent from Havana, Arkansas, through Oklahoma and up the main Arkansas into western Kansas, whence he crossed the panhandle of Texas to Handley's ruins. This survey mapped more than seventy sites. Mr. Franklin reported that ranchmen told him that there were other small stone buildings, or "Indian works," in out-of-the-way places farther up the river. Mr. Franklin paid particular attention to sites in Kansas along the main Arkansas, and on southern tributaries. He worked into northwestern Oklahoma, but found no particular change there. That is, the prevailing central Oklahoma types seem to continue up to the Cimarron river in the western panhandle of that State. His report is lengthy and he secured some 1500 specimens.

A search through the Peabody Museum library reveals very

scant reference to remains in the panhandle of Texas. Mr. Bandelier has stated that he heard of ruins on the upper Canadian, yet does not mention those farther down. I do not think he visited them.[1]

J. T. Eyerly, Esq., wrote a brief article in "The Archaeological Bulletin," some twelve years ago. He presented a small map of the Handley ruins, then known as "Buried City." Dr. Fewkes visited the Handley ruins and excavated in one of the sites, securing

FIG. 1.—General view of one of the larger foundations in Handley's ruins, Ochiltree county, Texas.

therefrom a skeleton. Mr. James Mooney told me in Washington, some years ago, that he had heard of ruins in the northwestern part of Texas, and expressed the opinion that they should be examined. So far as I can ascertain at present, nothing was published concerning these ruins save Eyerly's brief account, and none of the observers seemed to have reported the larger groups of ruins located farther up the river.

After a careful study of the notes and specimens secured by Mr. Franklin, it appeared that the region was important archaeologically. Therefore, in January of this year I went to Oklahoma

[1] *Archaeological Institute of America*, Series IV, Part II, p. 137.

and Texas and spent considerable time traveling through the Canadian valley and tributaries of that stream. Returning east the latter part of March, arrangements were made for the financing of a large expedition. The specimens discovered in the course of explorations were to be placed in the University of Pennsylvania Museum. The survey located itself on the ranch owned by Messrs. Sam and Oscar Handley, Wolf creek, Ochiltree county. We found there more than twenty buildings of stone, varying from six or eight to twenty-three meters in diameter. (See fig. 1). The upper portions of all these have disappeared, and only the foundations remain, and these were from one to slightly over one meter in height. Ruins are scattered along a level plain, convenient to the creek, and covering approximately one square mile in area. Five of these were excavated, and the results added to those of the first and second surveys. Something like three hundred specimens were secured, chief among which were two restored vessels, one nearly complete vessel, and numerous metates, grinding stones, small minute arrow heads, stone knives, bone tools, etc.

About twenty-five miles southwest, on the main Canadian, is a similar group on the ranch of Archie King, Esq., and now named King's ruins. Farther up the river at Plemons are Cottonwood and Tarbox creeks. On both of these are more buildings or foundations than occur at either King's or Handley's. The Cottonwood and Tarbox ruins almost join, the creeks being no more than two miles apart, and the buildings and graves extend back from the cañon edges a considerable distance on the plain. In most of these structures the stones have been placed on edge and the space between outside and inside of wall filled with earth intermingled with small stones. The thickness of the walls ranges from one-fourth to one-half or occasionally two-thirds of a meter. (See fig. 2). All are in ruins, the stones scattered, and accurate observations difficult to make. In some of the smaller structures there appears to be a slightly raised ridge of hard earth. On, or in, this large stones were placed on edge. The interior was excavated somewhat and there is a distance of one to one and one-half meters from the inside floor to the tops of the larger stones. Still farther up the river, on Ante-

lope and Dixon creeks, about twenty-five miles north of the town of Panhandle, are other groups of ruins. In the valley of Antelope, on a second terrace, stand the foundations of a building nearly fifty meters in length. Proceeding to the Landergin ranch, forty miles north of Amarillo, is yet another group. This one is somewhat

FIG. 2.—Excavation to base of the wall of a building on Handley's ranch, showing stones in position.

different from the others in that it is situated in a high pinnacle approximately one hundred meters above the plain. The space upon the summit is less than an acre in extent, and there are twenty-two foundations, nearly all of which are circular. There is no water on this summit and the nearest spring is more than a mile distant. A creek, half a mile away, is dry save during the rainy season. At the time the hill-top was occupied these people may have been able to secure water, but our party was unable to find any trace of a spring nearer than the one mentioned. An aged Mexican (nearly 80) named Isabel, living at the ranch, states there is no water save that spring, and that the Kiowa and Comanche and Apache, at various times, camped on the hill-top for protection. He visited them fifty or sixty years ago. They found the ruins there, but used the stones in building low walls, fireplaces, etc. He says they did not make foundations for their lodges.

The expedition of 1920 did not extend operations beyond the Landergin ranch for the reason that it had collected sufficient photographs, specimens, and data for a preliminary survey of the region. We were informed by cattlemen, who are familiar with the range between Amarillo and the Pecos valley in New Mexico, that similar ruins continue almost to the head of the Canadian.

FIG. 3.—Figure cut in sandstone bluff, Sam Hallock's ranch, Cimarron county, Okla. Thought by Thorbun to represent a Spaniard in armor.

The pottery becomes more of the Pueblo-Cliff Dweller type, and in some of the ruins farther west, painted pottery is said to be found. I am willing to accept this, since all information given us by cattlemen was found to be correct.

As a result of the last expedition, we have mapped nearly one

hundred sites or places where aboriginal work was in evidence. In Meade county, Kansas, we found irrigation ditches covering some seven to nine miles. This is the farthest east that such have been reported, so far as I can ascertain. Meade county was not settled in early times, but at a comparatively recent period. Broken pottery, flint chips, arrowheads, broken metates, and those curious objects made of lava, common in Arizona and New Mexico, were found.

In the Oklahoma panhandle, along the Cimarron, we discovered several series of pictographs. (See fig. 3). Whether these are the most extensive in the United States, I do not know, but they are scattered through eight miles of bluffs, and upwards of three hundred were noted by Mr. Johnson, Professor Thoburn, and myself. Quite a few were photographed and others drawn by us. They are com-

FIG. 4.—A ruin on Cottonwood creek, with graves in the foreground.

mon, we are told, along the river for miles. All should be studied and copied. Many of them were exceedingly well executed, are spirited, full of action, and evince no little artistic ability. Some of them are in color, others cut in the stone. Vandals have carved their names over and near many of these pictographs. The state of Oklahoma has been requested to take action to insure their preser-

vation. One large buffalo is painted life size, and will compare with the poorer of the paintings found on the walls of French caverns. We placed signs some distance from the pictographs, requesting people not to deface these remarkable examples of aboriginal pictorial art.

The survey of 1920 has opened a new field in American archaeology. (See fig. 4). On Cottonwood and Tarbox creeks alone, there are one hundred and nine stone graves in one group and more than

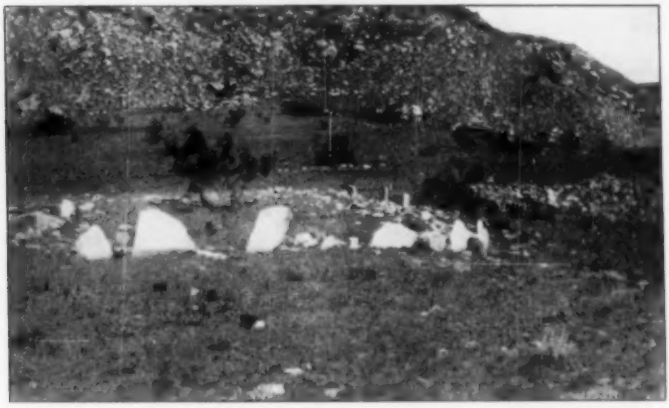

FIG. 5.—A large ruin on the second terrace, Antelope creek.

forty buildings. On Antelope creek, ruins are scattered through three miles of mesa, second terrace, and lower terrace of the valley. All we could do was to photograph, measure, and excavate. We carried a crew of eight men, but did not have time to examine more than fifty or sixty graves, and we excavated in twenty or twenty-five buildings. The graves are somewhat like those in Tennessee. Many are lined with flat slabs, not a few have stones placed on edge enclosing a space two by one-half meters. At a slight distance they give the impression of white men's burials, yet they are strictly aboriginal.

Few regions in the Painted Desert present a more weird aspect than the Cottonwood, Antelope, Tarbox, Dixon, or Landergin sites.

(See fig. 5). There is little vegetation save cottonwoods in the valleys. The cañons are brightly colored by disintegrated rocks in red, brown, white, and blue shades. Most of the groups do not appear to have been visited by any one save the few cattlemen of the region.

From a little distance, the large flat stones, on edge, and spaced more or less regularly, give the impression of miniature Stonehenges. It is estimated that two or three winters' work are necessary to make proper explorations. Summers are very hot and there is a scarcity of good water, hence winter is the best season for field operations.

The results of our labors will be set forth in a volume devoted to the archaeology of the Arkansas valley, to be published at some future time, and it is therefore a little premature to offer conclusions, yet the writer would present a few observations based on the work done.

Beginning in central Oklahoma, we note a change from the general culture of the Mississippi valley tribes. The grooved axe almost disappears, and a notched hand hatchet takes its place. The pottery of the middle Mississippi valley group begins to change, and little of it is found beyond Havana, Arkansas; and practically none occurs at Muskogee, Oklahoma.

In central Oklahoma, the small mano stone appears in large numbers, and the pestle of eastern form disappears; this is significant, it seems to the writer. There is no sudden change, although a gradual one, until we reach Jackson's ranch, seven miles down Wolf creek from Handley's ruins. Here we find the first stone buildings, rectangular in form, and the stones placed on edge instead of laid flat. This is characteristic of most of the ruins in the panhandle that the stones are placed on edge. Man has not yet learned to build a good wall for his house.

Several characteristics are common to all of these structures. They are not large, they are rudimentary, and to the writer's mind they mark the beginning of architecture in stone in the Southwest. The fact that buildings are very small but seven miles from the larger and more developed group on the Handley's ranch,

is not surprising. The large group mentioned was probably constructed a few generations later than the first ones. Or, the people may have first built small settlements such as we found on Turkey and other creeks. Omission is made, in this article, of several lesser sites. As the people increased in numbers and perfected construction, the larger groups, such as Cottonwood, Tarbox, Dixon, and Handley's, came into being by gradual and natural evolution. (See fig. 6).

FIG. 6.—Large stones forming part of the walls of a building on Tarbox creek.

It is suggested to other observers that the many scattered stones found throughout the region, and especially those which still lie in circles, were used to hold edges of skin tipis, there being little wood outside of the valleys. The region is noted for very severe high winds and heavy stones are needed to hold down tent edges. From use of stones to hold down the tipi or brush-covered lodge, to use of more stones, is a step the intelligent Indian soon took. Abundance of suitable rock in the cañons and draws afforded him material. Thin slabs, a meter or more in length, set on edge, and other slabs to form a second row, gave the natives a foundation on which might be laid a low adobe wall. Between the stones he filled in earth. We do not know the nature of the roof, whether

it was composed of cottonwood timbers on which was placed earth, or of skins. It is quite apparent that we should not class all these stone structures as erected by a tribe which later perfected the Pueblo style of architecture on mesas, in valleys or cliffs. This, for the reason that many of the stone circles are not house foundations, and also that certain of the ruins do not contain enough stone to represent substantial foundations. A bird's-eye view of the several hundred buildings visited by the expedition, would indicate that they mark the transition period from residence in skin-covered tipis to—

1. Small foundations with pole and skin coverings.

2. Thicker walls, probably adobe construction above.

3. Stones laid flat—rudimentary Pueblo-Cliff Dweller construction.

The Pueblo people did occasionally migrate and we have some historical references to certain of them building in western Kansas. However, it is suggested that these Canadian valley ruins are not pure Pueblo. Next to architecture, the dominant factor of Pueblo-Cliff Dweller culture is the high ceramic art these people developed. It is but two hundred miles from the Landergin ruins to Pecos Pueblo, where the art was high. The able researches of Dr. Kidder and Dr. Guthe have proved this. It is unthinkable that Indians would lose, or discard, their skill as potters in traveling that distance. There has been no true Pueblo-Cliff Dweller pottery found on any of the sites visited by our party. Therefore, one is led to believe that the Canadian valley culture developed as it proceeded westward, rather than that these sites are mere outposts of those well-known Pueblo people.

As to the age of these remains, nothing positive can be affirmed. The King and Handley ruins are built upon a previous village site where buffalo, bear, antelope, and other bones have been found, one-half to one meter below the foundations. This is not where an overflow from a stream has occurred, although it may be due to a gradual wash from the hillsides, one hundred and fifty to two hundred and fifty meters distant.

None of the stones used in the houses were dressed, and there

are several openings or doorways in most of the larger structures. The floors were ordinary clay, hard packed. No turquoise and no ocean shells were found in the rooms. The oldest Apache, Comanche, and Kiowa were assembled in council five times during March, 1920, on the reservation near Anadarko, Oklahoma, by the writer. Upwards of a hundred Indians were interviewed. The older men remembered hunting buffalo in the panhandle, and some of them were in the Adobe Walls fight of 1874. They all stated that they had ridden over many of these ruins, but knew nothing concerning the building of them; that the old men of those days also were ignorant; they occasionally camped on top of the bluff at Landergin's ranch, but their people did not build the stone circles there.

I regret that space does not permit presentation of illustrations of more of the buildings and the pottery. It is not Mississippi valley form; it is not Pueblo; it probably marks the transition. Finally, there is nothing found indicating Mexican origin or Pueblo influence. On the contrary, so far as the writer can observe, we have a tribe originally living in the buffalo country and of "Plains Culture" status which changed as it spread westward up the Canadian. They also built irrigation ditches farther up the streams from their villages. As they moved farther away from the buffalo country they continued to change and develop until they established themselves in permanent villages—were no longer nomads—and finally became the Pueblo-Cliff Dweller people.

ANDOVER, MASS.

NOTES ON SHELL IMPLEMENTS FROM FLORIDA

By CLARENCE B. MOORE

CONSIDERABLE has been written[1] about the interesting implements of shell, made from conchs (*Busycon*) and from "horse-conchs" (*Fasciolaria*), which are found throughout almost the entire state of Florida and sparsely in neighboring regions, but are preeminent as to numbers and quality of make in the midden sites of the aboriginal Key-dwellers along the southwestern Florida coast between Mound Key in Lee county and Lossman's Key in the county of Monroe, inclusive. (See map, fig. 7).

Farther south among the Keys, on the peninsula, and among the beautiful islands below it, these midden sites do not extend, doubtless owing to the scarcity in their waters of the oyster, the principal article of diet among the eaters of shell-fish along the coast.

This season (1920) was spent by us along the southwestern Florida coast among many aboriginal sites, most of which were familiar to us. Among these was Sandfly, formerly described by us as Wiggins Key, on Sandfly Pass, near Chokoloskee. At Sandfly we found a change of ownership[2] and name and that a considerable amount of territory had been cleared since our latest visit in 1904, all of it a midden site covered with shells left from aboriginal times.

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[1] Charles Rau, *Smithsonian Contributions to Knowledge* 287, "The Archaeological Collection of the United States National Museum," p. 66, *et seq.*, fig. 257.

Frank Hamilton Cushing, "A Preliminary Report on the Exploration of Ancient Key-Dweller Remains on the Gulf Coast of Florida," *Proceedings of the American Philosophical Society*, vol. XXXV, no. 153, p. 38 *et seq.*

C. B. Moore, *Journal of the Academy of Natural Sciences*, Phila.: "Antiquities of the Florida West-Coast," vol. XI, p. 380 *et seq.*; "Miscellaneous Investigation in Florida," vol. XIII, p. 315 *et seq.*; "Notes on the Ten Thousand Islands, Florida," vol. XIII, p. 463 *et seq.*

C. B. Moore, "Notes on the Archaeology of Florida," *American Anthropologist*, vol. XXI, no. 4, Oct.-Dec., 1919, p. 401 *et seq.*

[2] Mr. C. T. Boggess, the present owner, kindly placed the entire property at our disposal for repeated search.

FIG. 7.—Outline map of the southwestern coast of the Florida peninsula.

Here, on one extensive field almost exclusively covered with oyster shells, we found only two or three shell implements. These were of the ordinary type met with along the southwestern Florida coast, where almost the entire shell is used and has holes, or a hole and a notch, in the body whorl, somewhat below the shoulder, permitting the handle to enter the shell almost at right angles.

In another great field, however, abounding with implements, practically all the shell tools made from the conch (*Busycon*) proved to be variants and had either one or two large holes in the top of the shell above the shoulder, and none intended for handles below it.[1]

About a score of these shell tools from Sandfly, variants so far as the southwestern Florida coast is concerned, were sent to Mr. Charles C. Willoughby who very kindly has written as to the method of their hafting, and has presented to us three specimens hafted by him as shown in the illustrations, two from Sandfly and one from near the eastern coast of Florida.

The shell implements you sent for examination fall naturally into two groups —those with a single hafting perforation, and those with double perforations for hafting. All of these holes, whether single or double, are in the outer whorl of the spiral body above the shoulder. A few other minor perforations or fractures occur in the body of some of the shells, either above or below the shoulder. Judging by the weathering of their edges, some are as old as the hafting perforations, and as you suggest, may have been made to free the fish from the shell. Others are clearly of later date, for their edges show less weathering and some of the holes and fractures are relatively recent. I do not think that any of these smaller perforations had anything to do with the hafting of the implements, but that all of the twenty-two shells sent fall within one of the two above-mentioned groups.

I find practically no evidences of wear caused by the haft in any of the implements. Some of the hafting perforations are quite symmetrical with smooth edges, others are more roughly fashioned. The smoother edges, however, seem to be the result of superior workmanship rather than of wear.

It will be noticed that nearly all of the smaller implements come within the first group, and have but one perforation for the handle, while in the second group two perforations are required to insure proper rigidity of the heavier tool. The method of hafting, illustrated in fig. 8, answers well enough for the smaller tool, as the

[1] To show how nearly universal was this rule we may say that besides many imperfect shell tools of this class, some having one hole in the top and some two holes, we got from this field, entire or almost so, twenty implements each having two holes above the shoulder, and fourteen having one hole so placed. But two exceptions were found, each having a single hole below the shoulder and none elsewhere.

upper portion of the spire prevents the looped end of the haft from working loose in the ordinary light work such as the implement would naturally be used for.

The looped end of the handle of the heavier shell, however, (fig. 9), would soon become loosened by the additional strain and turn upward over the shell's apex, were it not held securely by the binding which passes through the second perforation. Most of the implements have picklike narrow cutting edges. Some of the larger ones, however, have adze-like edges. All were ground while

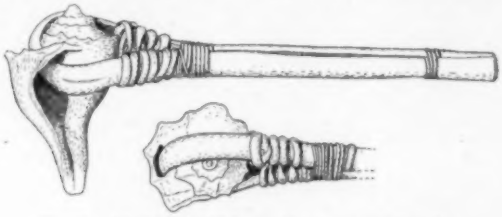

FIG. 8.—Method of hafting. About one-quarter size.

holding the aperture of the shell downward towards the abrading surface of the limestone, or other substance used for the purpose. The ground surface near the beak of the shell is therefore on about the same plane as the aperture. When the implement is being hafted, the handle can be adjusted so as to bring the cutting edge approximately parallel to it, or at right-angles, or into almost any position desired.

With the Indian's proficiency in such work, it would not take him long to select a shell of the proper size from which the fish had been previously extracted, grind

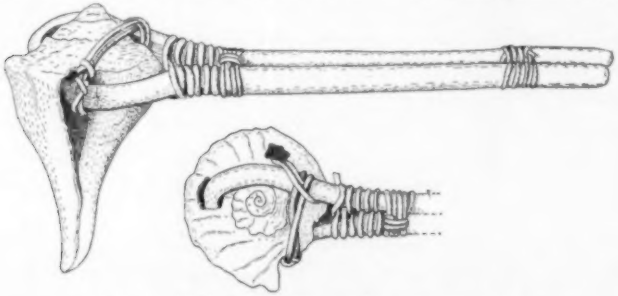

FIG. 9.—Method of hafting. About one-quarter size.

its beak to an edge, perforate the body whorl, and insert a haft. With a single blow of such a tool the body shell of a live *Busycon* could be quickly perforated to facilitate the removal of the fish, and I am inclined to think that many of the smaller tools of this type were often used for this purpose.

There is, of course, nothing new in the use of the looped handle such as I have

illustrated, except in its application to shell tools of this type for it was often employed by the Indians in hafting stone axes, mauls and clubs.

And now to take up another point in relation to tools of shell. Many of these implements have holes for the handle so carefully rounded that one is uncertain how they were made. A solution can now be offered as to some of them at least.

At Dismal Key we found a partly completed tool, evidently discarded on account of a crack accidentally developed in the making. The body whorl of the shell had been cut back about four inches from the aperture; part of the beak had been broken off to leave an end sufficiently thick to bevel into a cutting edge. The interesting point about the shell, however, is that in the place where a hole for a handle would be expected there is a round semi-perforation plainly made by pecking as shown in plate I. The margin of the hole later probably would have been ground into a regular surface.

On the top of this shell, above the shoulder, a small, irregular hole has been knocked through and this brings us to another question which, though discussed before,[1] may as well be brought to date.

We know that some of the natives around Key West, Florida, at the present time eat the conch (*Busycon*) and that, in order to aid in freeing the shell fish from its shell, they are accustomed to make a hole in it. Also we are told[2] that the aborigines of the West Indies made a small hole near the top of the shell (here the queen conch, *Strombus gigas*, is referred to) to aid in loosening the fish, and that the modern negro conch fishers there also break into the shell for this purpose but in different ways so that on finding these shells an investigator can determine by what class of persons the contents have been removed.

On such of those southwestern Florida Keys, however, where conch shells (*Busycon perversum*)[3] are found, often in great numbers, all brought there when their contents were used for food by the aborigines, while many of the shells have a small hole (in the top

[1] *Notes on the Ten Thousand Islands*, p. 463 *et seq.*

[2] Theodoor De Booy, *Contributions from the Heye Museum*, Number 9, p. 79. Reprinted from the *American Anthropologist*, vol. XVII, no. 1, Jan.-March, 1915.

[3] *Busycon carica*, having the opening to the right as one faces it, is not found on the western coast of Florida.

UNFINISHED TOOL OF SHELL SHOWING A METHOD OF MAKING THE HOLE FOR A HANDLE BY PECKING. (ACTUAL SIZE.)

as a rule), far greater numbers are without it. Therefore in this region we see that methods other than perforation of the shell often must have been practised to detach the fish.

We note also that these holes, which are small as a rule and of course are found in quantities of shells that have not been made into implements, on occasion served a secondary purpose when the shells were wrought into tools.

Cushing[1] in writing of his wonderful discoveries in the muck at Marco, a settlement at the northern end of Key Marco, tells us:

> Thus the stick or handle could be driven into these perforations [the large holes for the handle] past the columella in such manner that it was sprung or clamped firmly into place. Nevertheless it was usually further secured with rawhide thongs—now mere jelly—passed through one or two additional perforations in the head, and around both the stick and the columella.

The present writer is fortunate in having received as a gift from Mr. Cushing a conch-shell implement from the Marco exploration (fig. 10) having part of the wooden handle and showing plainly deep furrows indicating the former presence of the thongs described by him. The reader will see that probably starting from what is now a deep, central depression, evidently the location of a knot, a thong was carried to the reader's left, wound around the handle, brought back (as shown by the double groove), carried to the opening at the right where it was again looped around the handle, was led upward over the shoulder, part way not in contact with the shell, to the small opening above the shoulder, through which it was drawn around the handle inside the shell and then, returning through the same opening, was directed down over the shoulder to the starting point where the marked depression is apparent, and was there knotted to the other end and to the cross thong below.

On all the hundreds, or probably thousands, of shell implements, dry surface finds, with one exception, that we have examined, no such furrows were apparent and we believe the marks on the shell in question were created by corrosive action in connection with the decomposition of the thongs, the acid affecting the lime of the

[1] *Op. cit.*, p. 40.

shell lying in the wet muck, and were not the result of wear. The large tool found by us in the muck of the canal on Mound Key[1]

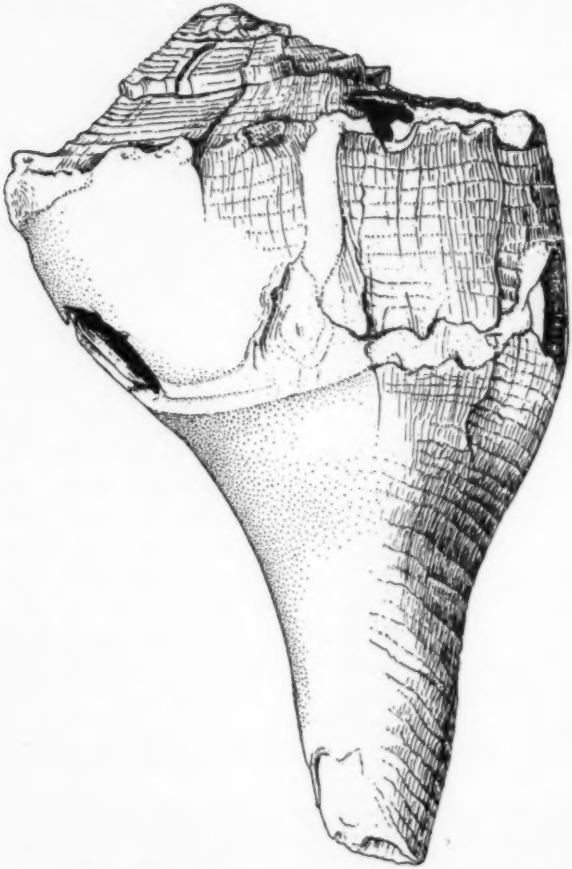

FIG. 10.—Shell tool from Marco, showing marks of thongs. Actual size.

perhaps had been dropped there without a handle and the accompanying thongs.

PHILADELPHIA, PA.

[1] *Certain Antiquities of the Florida West-Coast*, p. 388.

WORDS FOR TOBACCO IN AMERICAN INDIAN LANGUAGES[1]

By ROLAND B. DIXON

OF all the features of aboriginal American culture, the use of tobacco has long been regarded as one of the most characteristic. The idea of questioning its native character and antiquity could, in virtue of the mass of evidence indicating its use in very early times, hardly occur to anyone at all familiar with the results of American archaeology during the last generation. Professor Wiener, however, in a recent volume[2] has, in characteristically iconoclastic fashion, challenged this general conviction, and seeks to show, primarily on linguistic grounds, that not only are the words for tobacco over a large portion of the New World of West African Negro origin and ultimately derived from Arabic, but that the tobacco plant itself and the custom of smoking were unknown here until they were introduced, primarily by the Negro slaves brought over by the Portuguese and Spaniards, in the early part of the sixteenth century. Any question of the use of tobacco in America in pre-Columbian times is of course answered sufficiently and conclusively by the archaeological data and no amount of evidence that certain *words* for tobacco were of African origin could avail to prove the foreign introduction of the *plant*, in the face of the occurrence let us say of pipes or cigarettes in basket-maker or cliff-dweller sites, or of pipes in strata of typical Toltec culture in the Valley of Mexico. While therefore,

[1] In spelling the many native words I have usually transliterated antiquated forms, where necessary, according to more modern usage. In a few cases, however, where the original forms are not open to misinterpretation, I have left the older spelling, but enclosed the forms in parentheses. I have not burdened the pages with the voluminous footnotes necessary to refer in every case to the sources from which the various words have been taken, since these are for the most part well known to linguistic students. In a few cases where it seemed desirable, full references have been given.

[2] *Africa and the Discovery of America*, vol. I. Philadelphia, 1920.

Professor Wiener's theory of the African origin of the plant itself and the custom of smoking, is manifestly quite indefensible, it is perhaps worth while to examine critically the evidence which he presents for the foreign origin of certain tobacco words. It will, I believe, be shown, that not only is there little or no foundation for the belief that American Indian words for tobacco are derived from Negro or European sources, but that the author of the theory could hardly have arrived at his conclusions, if his investigation of the whole question had been less superficial and more sound in method. Incidentally, this survey of the various words in use throughout the New World for a single object, brings out a number of interesting details as to possible culture contacts between rather widely separated tribes, and raises a number of rather puzzling questions.

Discussion of the data may be prefaced by a few words as to general principles. Native names for native products may in general be expected to be confined to languages of a particular linguistic stock, and each stock may thus be supposed to have its individual stem or stems which will not appear in unrelated languages except in rare cases and then only where influence or contact exists or has existed. An introduced name for a foreign product on the other hand may be expected to show a distribution which is quite regardless of linguistic frontiers. Moreover, words of native origin in most cases may be expected on analysis, to show some relationship to stems of general meaning in the language; foreign words on the other hand, would either show no related stems, or only such as would naturally be derived from the exotic stem, and not of fundamental or general character. Similarities between words in unrelated languages naturally suggest borrowing, but this can not be regarded as proven, until analysis of the words has shown that they are not, after all, derived from quite different stems, and that the resemblance is thus only fortuitous.

Knowledge, however, of perhaps the majority of American Indian languages is still far too imperfect to enable us to do much in the way of analysis, and no one student can hope to be competent in this respect for all even of the languages for which we possess

reasonably detailed knowledge. In what fallows, therefore, I have merely tried to gather together and present the data, calling attention to such superficial, resemblances as seemed possibly significant, and only here and there attempting anything in the way of analysis.

Beginning the consideration of tobacco words with those of North America, it may be noted that the various linguistic stocks fall rather easily into two groups, (1) those which, with a wide area of distribution, employ two or more different stems, and (2) those, in general of more limited extent, which have but a single stem Taking this latter group first, it appears that the stocks which it comprises fall naturally into a number of geographic subdivisions.

The Southeastern group includes the Uchean and Timuquanan stocks, the word for tobacco in the former being *i*, in the latter *hini*. There is no certainty of any resemblance between these two forms, and the only suggestion of similarity with neighboring stocks is in the case of the Creek, *hitci*.

The Gulf group presents somewhat paradoxical results, since the Attacapan, Chitimachan, and Tunican stocks which have recently been united by Swanton show three quite different forms (Attacapa, *tsig*; Chitimacha, *net*; Tunica, *era*); whereas the Coahuiltecan, *a'h*, Karankawan, *ak-anum*, and Tonkawan, *baqa*, *ne-bax-kan*, seem to show considerable resemblance. On the other hand a second form, *naots*, given for Tonkawan, suggests possible relations with Chitimachan. With neighboring stocks there is a suggestion of relationship with the Caddoan, where the Wichita has *tahah*, and the Caddo, *yahah*.

The Southwestern group with Zuñi, *ana;* Keresan, *hami;* and Tanoan *ca*, *Le*, *tiōye* shows wide variability. The only form exhibiting similarities with other stocks is the Zuñi, which may be compared with Navajo-Apache, *nat'o*, and possibly (?) with the Siouan stem, consonant + *ani*, or the Karankawan *ak-anum*.

The Pacific Coast group is more widely scattered, but it too shows several examples of similarities. Beginning in the south we have Yukian, *waimil;* Yakonan, *tcuursen;* Kalapuyan, *kainoL;* Chinook, *KainoL;* Wakashan, *Lauk;* Tsimshian, *wundâ;* Haida, *gul*, *kwil;*

Tlingit, *gan(tc)*. Similarities are in all cases, as will be noted, between adjacent or supposedly related stocks. With neighboring stocks the only similarity noted is that of the Wakashan with the Bella Coola and Salishan tribes of Vancouver island. As the interior Salishan tribes all have a different term, it is obvious that the resemblance is due to the coastal tribes borrowing the Wakashan form.

The Plateau group, finally, like the one just considered, shows but few cases of possible relationship between its members. We have: Klamath, *katckal;* Shahaptian, *toh*, *tuwah;* Waiilatpuan, *huntc*, *fiänp;* Kutenay, *yaket*. The chief similarities with other stocks are Shahaptian *tuwah* with Hokan forms such as Shasta, *owa;* Yuma, *ova;* Pomo, *kuwa*. A curious instance of probably accidental resemblance is that of the Waiilatpuan (Cayuse) *huntc*, with the Tlingit, *gan(tc)*.

The results of this comparison of the words used for tobacco by the smaller linguistic stocks, show that in general each stock has its own peculiar form, although in a few cases borrowing may have occurred between adjacent tribes. For none of the words in these stocks has Professor Wiener asserted an African source, and none show any resemblance to his three primary Negro forms, except possibly the Tunica.

Turning now to the larger and more widely distributed stocks, we may begin with the Algonkian. The languages of which we have adequate information, have been grouped in four divisions, the Blackfoot, Cheyenne, Arapaho, and Eastern-Central, of which the last comprises the great majority. The words for tobacco in all the languages of this last division except the Eastern section, are derived from a common stem *sēma* or *asäma:* thus we have Cree-Montagnais, *tcistemâ;* Menomini, *nänimau;* Sauk, *seiman*, *sämon;* Fox, *Asämaw*^{a}*;* Kickapoo, *nessämon;* Shawnee, *lθema;* Ojibwa, *assäma;* Potawatomi, *sēma;* Ottawa, *sema;* Algonquin, *semah*. The phonetic variations are in accord with known sound modifications (Cree-Montagnais *st*, Menomini *n*, Shawnee θ = general Central Algonkian *s*). It is difficult to see any grounds whatever for supposing this Central Algonkian stem to have

been derived from the Negro *taba*, *tama*, *tawa*, for as initial *t* is common in Algonkian languages, there is no reason to expect a modification of *tama* to *sēma*. Since, moreover, the Arawak, word for tobacco (originally of "Negro origin") which, it is claimed by Professor Wiener, was brought overland from the Gulf to the Iroquoian tribes of Ontario and the St. Lawrence was not *tama* but *yuli*, *yari* giving rise to *yen*;[1] and since on the basis of his theory, the Central Algonkian tribes must have obtained both plant and name from this same source, there seems to be no possible way in which his *tama* stem of African origin could have reached them. Until the author of the theory explains how this could have occurred there can be no reason for giving the suggestion of African origin any serious consideration.

The Eastern sub-division of the Central Algonkian languages, derive their words for tobacco from a different stem, viz., Micmac, *tamāwā;* Abnaki, *udaman;* Maliseet, *tumawé;* Passamaquoddy, *dumawai;* Natick, *wuttamauog;* Narragansett, *ottomaok*. From this same stem, *tama*, *AtAmä*, the words for "pipe," and "to smoke" are also formed in these languages, and the words for "to smoke" in nearly all the rest of the Central Algonkian group. Professor Wiener might justly claim here, a virtual identity in sound between the Eastern Algonkian stem and his Negro *tama*—but, he has by his own statements denied himself the right to claim any relationship, since he states[2] that only the Brazilian *petun* forms were introduced by the French up the St. Lawrence, and explicitly declares that, in the region between this river and Florida, the words for tobacco "proceeded northwards from Virginia, where the oldest form of the word is an abbreviated Span. *tabaco* or Fr. *tabac*." This form *apooke* presents obviously no relation to the Micmac and other Eastern Algonkian words, so that again no evidence is presented for the foreign origin of the American stem.

The western languages of the Algonkian stock, differing as they do widely in vocabulary from those of the Central type, have, as might be expected, somewhat variant forms in their words for

[1] Wiener; *op. cit.*, p. 185.
[2] *Ibid.*, p. 191.

tobacco. The Blackfoot, *pistakan*, may conceivably be related to the Cree-Montagnais, *tcistemau*, and so ultimately derived from the general stem *sema*, *asäma*. The Arapaho, *sisawa*, and Gros Ventres, *seθawa*, are also, possibly related as is (?) the Cheyenne, *tsinimo*. That these western languages have, however, points of close contact with eastern forms is shown by the resemblance between the Blackfoot stem *-tsissi*, to smoke, and the Brothertown, *ni-tsisimu*, tobacco. The languages of the Lenapé and southern Atlantic coast tribes offer further interesting variants. The Lenapé themselves seem to employ a stem *-cate*, *-tcate* (*kshatey*, *koshate*, *gutschartai*, *shaate*) which suggests the Cheyenne-Gros Ventres stem for pipe, *-tca*, *-tsa*, or might on the other hand be compared with the neighboring Tuscarora and Cherokee, *tcahu*. On the other hand the word used for tobacco by the Virginia tribes and their relatives (*uppowoc*, *uhpook*, *apooke*, *hoohpau*) shows a stem from which these same tribes also derive their words for pipe (*uhpoocan*, *pawpekon*) and which is the basis furthermore for the words for pipe, not only in Lenapé (*hopoagan*) and Natick (*hopuonck*), but also in all the Central Algonkian languages, viz., Cree, *ospwagan;* Montagnais, *cpuagan;* Menomini, *ukpokan;* Sauk and Fox, *pwakan;* Kickapoo, *poakan;* Shawnee, *p'quaga;* Miami, *poakanoh;* Illinois, *poagan;* Ojibwa, *opwagan;* Algonquin, *opwagan*. That all of these are derived from *apooke* and that this is a mere apocopation of *tobacco* (in which Professor Wiener enigmatically says that the *t-* appears as the "pronominal suffix"!) is a theory which without further evidence is not worthy of consideration. The other suggestion,[1] that the Cree-Montagnais stem *pitu*, to smoke, is derived from the French, *petun* in turn from the Brazilian *petun*, while superficially more credible, is certainly open to some question. The general Central Algonkian stem for "to smoke" is, as has been pointed out, *AtAma*, *tama*, *AtAmä;* the Ojibwa and Algonquin, however, use a stem *sagas-*, while the Cree-Montagnais as stated above, have *pit* or *pitu*. The fact that the supposed loan word is found only in these two closely related languages which extend over a very large area, and is

[1] *Op. cit.*, p. 144.

unknown to the other Algonkian tribes of Canada who had at least an equal opportunity of borrowing the word, makes the suggested origin not a little doubtful.

The languages of the Muskhogean stock make use of two different stems for their words for tobacco. All except the Muskogee have a stem *hak* or *ak*, viz., Hitchiti, *ak-tcomi;* Natchez, *hag(au);* Alibamu, *hak-soma;* Choctaw, *hak-tcuma* from which the Muskogee form *hitci* seems to be quite different. With the words for tobacco in use by neighboring stocks, this general stem, *hak*, *ak*, seems to show no resemblance, although its similarity to the more remote Coahuiltecan, *a'h;* Karankawan, *ak-anum*, and Tonkawan, *ne-bax-kan* has already been pointed out. Professor Wiener in his search for evidences of borrowing of Negro words, found comfort in the Choctaw *hak-tcuma*, whose latter portion he identified as the stem for tobacco, and thought to be derived from the Mande *tama*, while the *hak-* he assumed to be the article.[1] In so doing he has "emptied out the baby with the bath," since what he thought to be the article is in reality the stem! That *hak-* was not the article a moment's reference to any Choctaw grammar would have shown, since the article follows and does not precede the noun.

Words for tobacco in the languages of the Siouan stock seem, in the majority of cases, to be derived from a common stem, of which the initial consonant is rather widely variable. Thus, Dakota, *tcañdi;* Assiniboin, *tcanti;* Kansas, *nahni;* Omaha, *nini;* Osage, *nanähu*, *pahni;* Ponca, *nini;* Iowa, *nanye;* Oto, *rane;* Winnebago, *tanina;* Ofo, *itcani;* Biloxi, *yani;* Tutelo, *yehni*. The Kwapa, *tadmi* and Mandan, *manacé* (Cf. Mandan *manainiduc*, tree; *manape*, leaves; Dakota, *tcañde*, *tcañwapa*) may also be derived from the same stem. The Hidatsa-Crow languages make use of a different stem *ope*, which, curiously, seems to be that used by the Catawba, whose word for tobacco is *umpa*. This stem seems also to be related to the Dakota *uñpa*, to smoke. Whether or not there is any relation between this Siouan stem and the general Algonkian stem for pipe, *upoa* it is hard to say; the resemblance of the Catawba *umpa* and the Virginia Algonkian *uhpook*, *apooke*, etc.,

[1] *Op. cit.*, p. 140.

is rather striking. Among the terms which Professor Wiener claims are derived from African originals, through Carib and Arawak, is the Biloxi *yani*. As this is so clearly a normal variation of the general Siouan stem, there is not the slightest reason to seek for it a foreign origin.

The Caddoan languages appear to have two different stems. The more northerly tribes have, Arikara, *nakuckanu;* Pawnee, *nawiskaru;* while the more southerly have forms which seem quite unrelated, viz. Caddo, *yahah;* Wichita, *tahah* or *weko*. The resemblance of these latter forms to the neighboring Coahuiltecan, Karankawan, and Tonkawan has already been noted.

In the languages of the Salishan stock, all the interior dialects make use of a single stem, viz., Lillooet, *smanih;* Thomson River, *cemen'eq;* Shuswap, *smanq;* Okinagan, *sma'n'uq;* Flathead, *sman'hu;* Coeur d'Alene, *semelkh;* Columbia, *smanhu;* which is also the basis of the words in two of the coastal dialects, *i.e.*, the Songish and Niskwalli, which have Clallam, *smanac;* Lummi, *smanuc;* Songish, *smanic;* Niskwalli, *smanac*. This wide-spread stem shows no apparent resemblance to forms in use in any adjacent stocks, but may perhaps (?) be compared with the Central Algonkian *sema, asäma*. The Blackfoot, however, who are actually in contact with the Eastern Salishan tribes did not use this stem, so that unless their form *pistakan* is a relatively recent term, it is difficult to see how the borrowing could have taken place. Although the great mass of the interior Salishan tribes have thus a common stem for tobacco, the coastal tribes, with the exceptions above noted, use other stems. The more northern tribes have adopted the Wakashan stem, Bella Coola, *La'uk;* Comox, *a'wak;* Cowichan, *ewauk;* while the Sqwamish and Nanaimo dialects appear to have an independent form, *spo'Len*. The two southern coast groups, the Chehalis and Tillamook also have words which, with one possible exception, seem to have no similarities with others either within or without the stock, viz., Chehalis, *stxlusoqua, kwlemihin;* Cowlitz, *kwalemutxlin;* Tillamook, *suxoötxlil, tsotxltxl*. It is possible that the Cowlitz form is related to the Nez Percé, *kelamut* pipe, itself apparently from "calumet" which is of European origin.

The words for tobacco in the Athapascan stock show a number of interesting features.[1] Beginning with the northern tribes we find one stem in use by the Kutchin tribes, viz., Loucheux, *tsetted;* Upper Yukon, *se'ei-i-ti-it;* as well as the Dog-rib, *tsedetti;* the Hares, *tse-etturi;* the Montagnais, *tseenttu;* the Slave, *sɛt'u, tceltohi;* and Chipewyan, *ts'ɛlt'ui.* In these the *-t'u* is probably the stem for "to suck." A second stem is found among the majority of the British Columbia tribes, viz., Sekané, *edeke;* Nahané, *dekkei, tseakh;* Carrier, *teka, tsabara;* Beaver, *atdegai.* Individual tribes in this northern group present still further variants: thus we have Kaiyukhotana, *tahkuna;* Tsetsaut (Ahtena), *k'a;* Sarsi, *'akatcinna;* Chilcotin, *tsulu.* Of these the Chilcotin seems to be related to the Chipewyan-Slave type, while the Sarsi may be compared with the Sekané-Carrier type so far at least as its first portion is concerned; the *-tcinna* probably means "stem of a plant." This is curiously similar to the *tsinimo* of the Cheyenne. Possibly the *'aka-* of the Sarsi and the *teka* of the Carrier, Sekané, etc., may be comparable with the Tlingit *gan.*

The Pacific Coast group of Athapascan languages, show rather more uniformity among themselves. Tlatskanai, the northernmost, has *tötcané* which appears to be unique, but the Umpqua *setxlio* and the Coquille, Tolowa *selyo* and Lassik *selyo* are practically identical and are analyzable into *sel* + *yo,* of which the latter is the stem "to blow." The first portion *sel-, set-* seems likely to be the same as the *ts'el-,* or *tset-* of the Chippewyan group, and if this suggestion is confirmed, we should have one single stem in use among tribes of this stock from the Mackenzie delta to California. Whether the Nongatl and Sinkyone *sin-yo* is also traceable to the same stem, is not clear. The Hupa and Kato forms, *mindeiltcwe* and *lit-tanoñ* show no relationship at all to other forms and are probably the result of circumlocutions devised on account of the death of some person whose name involved the stem formerly used for tobacco.

The words for tobacco among the Southern Athapascans,

[1] For much information in regard to Athapascan forms I am indebted to the kindness of Dr. P. E. Goddard.

Navaho, and Apache, *nat'o;* Jicarilla, *nat'odi*, are practically identical, and like the Chipewyan group in the north, are compounds of the verbal stem *-t'o* to suck. It is at least interesting, and perhaps significant, that the Zuñian word for tobacco is *ana*.

The apparent relationship of the Pacific Coast Athapascan *sel-*, *set-* to the Chipewyan-Kutchin *ts'el-*, *tset-* would seem to make it probable that the knowledge of tobacco had already been acquired before their separation, and further, would suggest that the Pacific Coast group were associated more with the Plains-Mackenzie than with the Rocky Mountain group of Northern Athapascans. Moreover, the fact that the Kutchin tribes of the upper Yukon use the same stem as the Chipewyan, Dog-rib, etc., would indicate that the use of tobacco spread into the upper Yukon region from the eastward. Lastly, the fact that the Navaho and Apache have a different stem than any of the other branches of the stock (*i.e.*, the use of *na-*) might be taken as evidence that the separation of this southern group took place before the use of tobacco was known.

In the Iroquoian stock, the Five Nations and probably the Hurons made use of a common stem (*o*)*yengw*, (*o*)*yenkw*, viz., Mohawk, *oiengua;* Onondaga, *oienkwa;* Cayuga, *oyeangwa;* Seneca, *oyanqua;* Huron[1], *oyngoua*. The Huron forms given by Sagard (*testena*, *tistenda*, *ayentaque*) are obviously from other stems, and will be considered later. Whether the form (*quiecta*) given by Cartier as in use at Hochelaga is from the same stem is uncertain. Transliterated into phonetic spelling, Cartier's word would probably be something like *kiyekta* (Cf. Iroquois, *ki-enkwa-thas*, *ki-yenkwa-t'as*, to put tobacco in one's pipe) in which the *-yek-* may be a dialectic form of *yenk*^{w}. The southern Iroquoian tribes derived their words for tobacco from a wholly different stem, viz., Tuscarora, *tcarho*, *tcehra;* Cherokee, *tcarhu*, *tsalu*, which may perhaps be compared with the Onondaga, *watcrota* (*wachrota*), to smoke.

Professor Wiener in discussing the Iroquoian tobacco words seems to have gone astray in consequence of insufficient investigation of the facts. He states in the first place that Sagard's Huron forms for "tobacco," *ayentaque*, *testena*, *tistenda*, and *ayettaya*

[1] As given by Lahontan, Thwaites edit., II, p. 748.

"I smoke," are "all from the root *yen*"[1] from which he also derives the Onondaga and Mohawk forms (*ojenqua*, *oienkwa*, etc.). A little further investigation, however, would have shown that the stem for tobacco is probably not *yen* since this enters into the formation of a number of words of quite unrelated meaning, viz., Mohawk, *gaienrha*, (*oienguara*) smoke; *oiente*, (*gaienta*) word; *gaienseron* to decorticate; *gaienton* to strike; *gaienθon* to own a field; *gaienteron* to know, etc., etc. It seems probable that the stem for tobacco must end with a guttural. Turning now to the Huron words as given by Sagard, it is clear that the problem cannot be settled in such an off-hand manner as that employed by Professor Wiener. It is fairly obvious that *testena*, *tistenda* are not derived from either *yen* or *yenkʷ*. The form *ayentaque* like the Iroquois terms cannot well be from a simple stem *yen*, but rather from *yent*, *yenta*. This it may be observed, is the Iroquois stem for wood (*oiente*, *gaienta*) to which the Huron form, although rather variant (*ondata*, in composition *-inda-ta*), seems related. The Huron word for "I smoke" is, moreover, probably not derived from this stem at all. Sagard gives a number of forms, viz., *ayettaya*, *agataya* "I smoke"; *etaya* "give me something to smoke"; *ataya* N. "N. smokes"; *sateya* "smoke!" Obviously, these are not derived from any stem *yen* or *yent*. Chaumonot, whose knowledge of the Huron language was far better than Sagard's, gives *atayen* "to smoke"; *etayak*" I smoke"; *eyetaya* "I shall smoke"; *te yetayan'de* "I am not smoking." The stem involved in all of these forms is pretty clearly something like (*a*)*taya* or (*e*)*taya* which may not improbably be connected with (*a*)*teyen* to burn. From what has been said, it is clear that the theory of the derivation of the Iroquoian and Huron words for tobacco from a stem *yen* derived from the Negro through the Arawak *iouli*, *yari*, *yeury*, improbable as it is on other grounds, is also indefensible on the linguistic side.

The two recently advocated Californian stocks, the Penutian and Hokan, each of which combines a number of what had previously been regarded as separate stocks, show little uniformity in their words for tobacco. The Penutian has more separate stems than

[1] Wiener, *op. cit.*, p. 145.

the number of former stocks of which it is composed, viz., Maidu, *pan;* Wintun, *lol, bomit;* Miwok, *kaiyau, kasu, kahu;* Costanoan, *mat(er), oya, sawan-s;* Yokuts, *sokon, tcani, baum.* Within the stock it seems reasonable to connect the Maidu *pan* with Yokuts *baum* especially since the Sierra dialects of the Miwok which lie between the two, have *paumma* for pipe. With this may also be placed the Southern Wintun *bomit.* The same stem, apparently, is found further afield, namely among the Shoshonean tribes lying to the eastward, where the Shoshone-Comanche and Mono-Paviotso have *pamo, pamu.* If the common origin of these forms is admitted, the question naturally arises as to whether the Shoshonean or Penutian tribes were originators of the stem, but this can hardly be answered with certainty as yet. Other comparisons which might be made are Costanoan, *oya,* with Yukian, *woyol;* Yokuts, *sokon,* with Chumashan, *co(x)* and Pomo, *sako, saxa.* The Miwok, *kasu, kahu* and Esselen, *k'aa,* is more doubtful, and the close similarity of Yokuts *tcani* and Dakota *tcañdi* is probably wholly fortuitous.

In the case of the Hokan stock there is more probability that the majority of the words for tobacco are formed from a common stem. Thus we have Karok, *-hera;* Chimariko, *uwuh;* Shasta, *owa;* Achomawi, *op';* Atsugewi, *ohpi;* Yana, *mohu;* Pomo (N., C., E. and N. E.) *saxa, saka, sako;* (S., S.W., S. E.) *kawa, tom-kowa;* Chumashan, *co(x);* Salinan, *talam;* Esselen, *k'aa;* Yuman, *uba, ova, auva, auba, omp;* Seri, *api;* Washo, *bankuc.* The Washo form suggests a connection with the Penutian-Shoshonean *baum, pamo* which, since the Washo are entirely surrounded by these tribes, would not be improbable.

In connection with the Californian words for tobacco, mention must be made of the term given by Fletcher as obtained on Drake's voyage in 1579, to which much importance is attached by Professor Wiener.[1] Fletcher states that the Indians brought as a gift, bags of an herb which they called *tabah* or *tobah.* This is declared by Professor Wiener to be evidence that tobacco and its name (derived in this instance from Span. *tabacco*) had been introduced by Spanish or Portuguese visitors prior to Drake's visit. The consensus

[1] *Op. cit.,* p. 141.

of opinion places the location of his stay at or in the vicinity of Drake's Bay, which lies squarely within Miwok (Penutian) territory. The dialect spoken in this vicinity has for tobacco the word *kaiyau* in which it would require a very lively imagination to see an origin from the Spanish *tabacco*. It may be noted, however, that the coastal Pomo (Hokan) dialect adjoining the Miwok to the north, has *kawa* for tobacco which might have been heard by Fletcher as *tabah*. If this is the source of his word, then either the place of Drake's stay must have been farther north than has been thought (and on account of the character of the coast this is in the highest degree unlikely) or the southern limit of Pomo speech must have moved northward considerably since the end of the sixteenth century, for which supposition there is little evidence. It is of course also possible, that the plant in question was not tobacco, although in view of Dake's statement that the Indians brought "tabacco" this is not likely. That tobacco and its name could have been introduced prior to Drake's visit is extremely improbable. Cabrillo was the first explorer of the Californian coast north of San Francisco. On his voyage in 1542 he neither landed nor had any contact with the Indians in this region, and no other explorer or visitor is known to have been in the region between this date and Drake's visit, so that introduction of the plant and its name would seem to be quite doubtful. Moreover, the fact that the two species of tobacco (*i.e.*, *Nicotiana Bigelovii* and *N. attenuata*) used by the Californian Indians were local and quite different from those in use in Mexico and the eastern United States would in itself be sufficient evidence for any botanist of the impossibility of Professor Wiener's claim.

With the Shoshonean languages, we come to the northernmost member of the now pretty generally recognized Uto-Aztecan stock which includes, besides the Shoshonean, the Piman and Nahuan languages. The Shoshonean branch is far from employing a single stem for its words for tobacco. The Shoshone, Bannock, Mono-Paviotso, and Comanche all have a stem *pamo*, or *pamu*, whose similarity with Penutian forms has already been pointed out. The Ute-Chemehuevi words are derived from a stem *kvap*, *kwap* which appears to have no cognates anywhere. With the Southern

Californian dialects we come to what is apparently the general Uto-Aztecan stem *piva*, only the Kern River tribes differing in having a form *cokont* derived from the Yokuts *sokon*. The general Uto-Aztecan stem is found among the Hopi and throughout the Piman and northern Nahuan languages as far as Durango, viz., Hopi, *piva;* Pima, *böv;* Papago, Opata, *viva-t;* Tarahumare, *wipa-;* Yaqui, *vivam;* Tepehuane, *vivai*, *virai*. In the Cora and Huichol further south this becomes *ya*.[1] In the Nahua itself the word for tobacco seems clearly to be *ye-tl*, the stem of which may well be related to the Cora-Huichol *ya*.

The evidence for this Nahuan form, however, requires a brief discussion, since Professor Wiener has declared[2] that the word "had no special meaning" and was probably derived from the Negro through the Arawak *yuli*. As he states, Olmos seems to be the first to use the term *yetl* which he translates "sahumerio," *i.e.*, perfume, incense. In Molina's dictionary the word itself does not appear except in the form *picietl* defined as "an herb like henbane, which is medicinal." Professor Wiener goes on to point out that Hernandez[3] does not use the word *yetl* in the edition of 1615, but in that of 1651[4] gives it as the equivalent of *picietl*, saying further that it is used in the making of cigarettes. So far Professor Wiener is on safe ground, but makes an unpardonable error when he states that *yetl* is a "back formation fron *picietl*" which is itself "an un-Mexican formation, for *pic-* does not occur in any other word whatsoever."[5] As a matter of fact there are four other words which immediately follow *picietl* in Molina's dictionary which contain the stem *pic-*, viz., *piciliui* to become small (not "to crush, to triturate" as Professor Wiener gives it); *piciloa* to waste or diminish; *piciltic* small, as of objects like pebbles or pearls; *picqui* a compact, solid thing. Other examples of the use of the

[1] Sapir: *American Anthropologist* (N.S.), XVII, p. 110.

[2] *Op. cit.*, pp. 150, 155.

[3] F. Hernandez: *Cuatro Libros de la naturaliza y virtudes medicinales de las plantas y animales de la Nueva España.* Morelia, 1888, p. 136.

[4] *Nova plantarum, animalium et mineralium mexicanorum historia. Romae*, 1651, pp. 173 sq.

[5] Wiener: *op. cit.*, p. 150.

same stem are the reduplicated forms *pipica* to drop a liquid drop by drop; *pipiciltic* a small seed or similar object and also *picicitli* a species of small bird. In saying, therefore, that *pic-* "does not occur in any other word whatsoever" except *picietl* Professor Wiener is guilty of an extremely careless misstatement and misrepresentation of the facts. It is fairly obvious that *pic-* is a stem connoting the general idea of "smallness," and that therefore we are led to suppose that *pic-ietl* (*pic-yetl*) probably means "small or dwarf *yetl*." If we turn to Hernandez this supposition is immediately confirmed, for on the same page to which Professor Wiener refers, it is expressly stated that there are two varieties of the plant, one called *picietl*, the other *quauhiyetl* of which the former or ordinary variety is a small plant "two palms in height," whereas the latter (whose name means tree-*yetl*) is much larger, growing as tall as a lemon tree. This demonstrates very clearly the existence of *yetl* as an independent word, and that it can be nothing else than tobacco is shown by Hernandez's explicit statement that the small variety is the same plant which is called *tauacco* in Santo Domingo, and that it is used with other aromatic substances in the making of cigarettes. Further confirmation of the existence of a word *yetl* may also be seen in the modern Mexican term *yetlalcingo* (for *yetla-tzin-co*) meaning "tobacco plantation."[1]

For the little known and extinct languages of northeastern Mexico, no data are available, but from the four stocks in the great enclave in Nahuan territory, material is at hand for three, viz., Otomi, *yiy;* Totonac (Tepehua) *llui, huxcuti;* Mayan (Huasteca) *mai*. Of these the only one which may be compared with forms in neighboring stocks is the Otomi, which is not very remote from the Nahuan *ye-* and Cora-Huichol *ya*. From the Zapotecan stock we have forms from the Zapotec, *gessa* and Amuzgo, *(ts)o-kohnu;* from the Zoque we have *otsi, tsaivi* and Mixe, *huuk, hvik* none of which appear to show notable resemblance to terms in other languages. The Tequiztlatecan, which has recently been suggested as a remote outlier of the Hokan stock, has *ame*.

The Mayan stock, all of whose members except the Huasteca

[1] Robelo: *Diccionario de Aztequismos*, Cuernavaca, 1904, p. 415.

occupy a solid block from the Caribbean to the Pacific separating the languages of Mexico from those of Central America, has three different terms for tobacco. The Tzendal and Chicomulteca in Chiapas and the Kekchi and Pokomam in central and southern Guatemala use the form *mai* which is that in use, as already pointed out, by the Huasteca in Vera Cruz. The Chuje, Jacalteca, Mam, Aguacateca, Zutuhil, and Uspanteca occupying the area in Guatemala between the two *mai*-using groups, employ the stem *sic*. The Chontal, Maya, Chol, and Chorti who occupy all the northern portion of the stock area on the other hand have a stem *kutz*. On the basis of the tobacco words, therefore, the outlying Huasteca seem connected with the Chiapas and Guatemalan tribes rather than with those of Yucatan.

In the region of great linguistic diversity lying between Guatemala and Panama, data on words for tobacco are available for all the important languages except those of northern Honduras (Jicaquean and Payan) and the southern Nahua dialects. The terms found are as follows: Lencan, *wa, yahua;* Subtiaban, *rande;* Matagalpan, *wili, wilin;* Chiapanecan (Dirian) *nemurema*. Of these the Lencan and Matagalpan may well be compared with the Cunan *wala*. The Ulvan *aka* and Mosquito *twaka* are obviously connected, and the latter at once suggests an origin from *tobacco*, yet Brinton[1] gives a quite different word for the Mosquito, viz., *u* which would look rather toward the *wa, wala, wili* forms just noted. It is to be observed, moreover, that *Twaka, Taoca* is a well known name of one of the Mosquito tribes, and that the word also appears as the name of one of the rivers in their territory. The question therefore is one which requires further investigation before it can be regarded as settled.

With the first of the Chibchan tribes, we cross definitely the ethnographic boundary into South America. In treating the data from the southern continent, the material from the Pacific Coast languages will first be considered, followed by those of the Atlantic drainage.

The Chibchan stock is divided into an Isthmian and a con-

[1] Brinton: *Proc. Amer. Philos. Soc.*, XXXIV, p. 413.

tinental section by the Chocoan tribes who occupy the northwestern portion of Colombia. All of the Isthmian group (except possibly the Guaymi) form their words for tobacco from a stem *dua, tua*, viz., Guatuso, *tua, tuah;* Boruca, *dua;* Bribri, *deua, dawah;* Cabeçar, *duwa;* Terraba, Tirribi, *dua, dowo;* Dorasque, *dua, durni*. The Guaymi form is given as *so*, but this is probably the term for "to smoke," cf. Dorasque, *dua suluk*, "to smoke." Professor Wiener declares unhesitatingly that all these forms are derived from *tabacco* introduced by the Spanish and Negroes from Hispaniola. While such an origin is possible, yet in view of the existence of the neighboring forms like *wala* of the Cunan tribes, whose language is supposed to be a probable member of the Chibchan stock, the conclusion ought not to be jumped at that these *tua* forms are certainly from *tabacco*. The continental Chibchan languages present a complete divergence from the isthmian group and, moreover, do not agree among themselves. Thus we have Köggaba, *noai;* Chimila, *kööröka;* Paez, *mueihi;* Chibcha, *hoska*. If we accept the wider limits for the stock recently proposed by Rivet, we find for the Barbacoan, Cayapa, *tago;* Colorado, *taako* forms which more certainly look as if they were derived from *tabacco*. The coastal districts of Colombia and Ecuador might *a priori*, be regarded as the one area in the New World outside the northern and southern extremities, where tobacco would be least likely to have been in use in pre-Columbian times, so that a term of introduced origin might perhaps be expected here. The word found here preserves, as does the less certain Mosquitoan *twaka* and the Eskimo forms, the gutteral sound present in the original, a fact certainly in favor of its origin from *tabacco*. Further evidence on this point will be referred to later, but as bearing on the whole question it may be noted that the Chocoan tribes who occupied the whole of the northern Pacific coast of Colombia, have a term for tobacco *ade* which seems quite unrelated to neighboring forms and is not open to the suspicion of an origin from *tabacco*, while the same thing is also true of the Esmeraldan stock on the southern Ecuadorian coast, who have the term *kanca*.

Whatever may be the ultimate decision as to the precise relation-

ship between Quechua and Aymara, these two languages of the old Inca state both employ the same word for tobacco, viz. *sairi*. There is considerable evidence of a wide dispersion of this term or rather of what may be its essential stem, in the area east of the Andes, but these instances of possible transmission can more conveniently be considered in treating of the languages of the Atlantic drainage. Professor Wiener, in seeking African origins for American tobacco names, derives the Quechua-Aymara *sairi* from the Negro *sira* meaning snuff,[1] basing his argument on Herrera's reference to "a load of shipwrecked Negroes" who in the early sixteenth century "had landed in Peru, where they formed a settlement with the Indians." It is quite evident, however, that Professor Wiener has again been in too much haste to prove his theory, and has failed apparently, even to consult a map to note where on the coast of "Peru" these Negroes came ashore! Had he done so, he would have found that the Cojimies river (as it is now called) where the refugees were said to have landed is in northern Ecuador, in Barbacoan, *i.e.*, Chibchan territory and quite out of contact with Quechua speech. His theory thus, turns out to be baseless, in so far as he is seeking a source for the term *sairi*. It may be noted, however, that the Cojimies river is approximately the southern boundary of the Barbacoan group of Chibchan languages, among which group the presence of a form *tago*, *taako* has already been noted. Possibly Herrera's Negro refugees are responsible thus for the introduction of the obviously tobacco-like term, having brought it with them from Hispaniola. This question remains, however, an amusing little puzzle.

The Araucanian term for tobacco presents a case of some little interest. The older forms of the word are given as *ptem*, *puthem*, *puthen*, while the modern form is given by Augusta[2] as *pɛtrem*. Lenz[3] and others have regarded the word as probably derived from the Guarani *pety*, and since Professor Wiener regards this latter as of Portuguese origin, he enthusiastically follows and adopts this

[1] *Op. cit.*, p. 186.

[2] F. J. de Augusta: *Gramatica Araucana*, Valdivia, 1903, p. 391.

[3] R. Lenz: *Diccionario Etimologico de las voces chilenas derivadas de lenguas indijenas Americanas*. Santiago de Chile, 1904–10, p. 616.

view of the source of the Araucanian word.[1] Although it must be admitted that the similarity between the Araucanian and Guarani words is sufficiently striking to warrant the possibility of their relationship, yet there is not a little to be said on the other side. For it may be noted that there are in Araucanian several words beside that for tobacco which are formed from the same stem, (viz. *pt·en*, *puthen* to burn; *puthon* to perfume, fumigate, smoke; *puthocan* to make a smoke fire). Now while it would be possible for the word for tobacco to be derived from a stem from which come the ordinary words for "to burn," "to make a smoke fire," etc., it would be almost incredible that these latter words of wide and general meaning should have come into existence only, as Professor Wiener says, after the use of tobacco became known, particularly if this was a knowledge gained so late as the sixteenth century.

In speaking of this assumed late origin of the Araucanian term from the Portuguese through the Tupi-Guarani, Professor Wiener seeks to justify and account for its transmission by stating that "as Chile was a part of the Province of Rio de la Plata, the Guarani-Tupi word for "tobacco" and its derivatives naturally were transferred to the west." This statement is, apparently, quite unwarranted as Chile proper never was a portion either of the Viceroyalty of Rio de la Plata (which did not even come into existence until 1776 or nearly two centuries *after* Valdivia wrote his account of the Araucanian language); or of the earlier province which, organized in 1620, was separated from Chile proper by Tucuman and Cuyo. The theory of the late introduction of the Tupi-Guarani term through Spanish influences has obviously, little or nothing to be said in its favor, but this is not to deny that such transference might not have taken place long before the advent of Europeans; indeed the striking similarity of the words in the two languages would naturally lead to this inference. Yet in this connection, it must be observed that the two stocks were not in contact in historic times, being separated from each other by a wide extent of territory occupied by Calchaquian, Puelchean, Guycuruan and Charruan tribes. If transference had taken place

[1] Wiener: *op. cit.*, p. 185.

westward from the Guarani, one might expect to find traces of the fact in the words for tobacco used by these intervening tribes. Unfortunately we have data only from the Puelchean (?) and Guycuruan stocks. The word for tobacco among the former was *putroquin* of which the first portion would seem to be closely allied to the Araucanian especially if we consider the modern form *petrem*. The Guycuruan forms, *asareh*, *eserike*, etc., are obviously of quite different origin. The whole question is a rather involved one, and all that can be said with any certainty is, that any transmission which may have occurred was probably long anterior to the first appearance of Europeans in America.

The Tupi-Guarani is one of the most widely distributed stocks in South America and affords, with the Arawakan and Cariban languages, data of importance in the study of tobacco words. The Tupi-Guarani are divided into a number of more or less separate groups, from all of which except the Bolivian tribes we have material. The Guarani used a form which in phonetic rendering, would seem to have been approximately *petü*^{n}, while the Tupi of the Brazilian coast (who were probably offshoots of the Guarani that had migrated northward) employed a closely similar word, *petema*, *pyty'ma* (phonetically more nearly *pütüma*) *petun*. Of the Central Brazilian group the majority appear to have used closely related forms, viz. Apiaka, *petema*, *penteu;* Cayova, *pytyhla;* Yuruna, *poutima*, *pouitima;* Kamayura, *petun*, *petüm;* Aueto, *pä*, *päh*, *bē*. The Maues and Mundurucu however have apparently quite different forms, viz. *sovo* and *hĕ*, *ĕĕ*. The upper Amazon group and the tribes of the Guiana borders, again show close similarity to the original Guarani type, viz., Cocamas, *pitema*: Omaguas, *petema*, *pitihla;* Oyampi, *petum*, *petoun;* Emerillon, *petime*. On the whole, the many tribes of this stock show a striking degree of uniformity over a great area, ranging from southern Brazil to the Guianas and from the eastern slopes of the Andes to the Atlantic coast at Cape S. Roque. Except for the already discussed case of the Araucanian resemblance, the Tupi-Guarani stem shows no similarity to other languages, and seems with one or two insignificant exceptions, to be confined to members of the stock alone.

This typically Tupi-Guarani stem, Professor Wiener declares[1] to be of modern origin, being derived he says from the Portuguese *betume* a word meaning "any pasty substance" but transferred according to his theory to tobacco. For this transfer or for the use of *betume* to mean tobacco, no evidence whatever is given; until it is given, one may be pardoned for regarding the theory as utterly baseless. Yet, even if clear evidence of the use of the word by the Portuguese with this meaning were brought forward, it would be incumbent on the proponent of the theory of its being the source of the Tupi-Guarani stem, to show how this borrowed stem could have obtained the distribution which it has. That in its spread it should be so strictly selective as to be adopted only by Tupi-Guarani tribes, and should reach far distant and isolated tribes without appearing among their neighbors, is very hard to believe, and very clear proof for such a theory would be necessary for its acceptance. Until then, some valid evidence is presented in its favor, the suggestion that the Tupi-Guarani words for tobacco were all derived from the Portuguese *betume* must be considered as quite unproved.

The Cariban stock vies with the Tupi-Guarani in its wide distribution, and like the latter, is spread over one large, continuous area and numerous smaller, isolated ones. Among the tribes occupying the large, continuous area, (covering a large part of Venezuela, the Guianas and northern Brazil) the word for tobacco appears in two forms, viz.: Cumanagota, *tam*, *tamo;* Cariniaco, *tamoui;* Carib, *tamoh;* Galibi, *tamoui;* Roucouyenne, *tamoui;* Apalai, *tamoui;* Akawai, *tamui;* Pianacoto, *tamoui;* Arara, *tamoui;* and Tamanaca, *kauwai;* Arecuna, *kauwai;* Macusi, *kawai;* Maquiritari, *kauai;* Paravilhana,*kauwai;* Azumara, *kawi;* Purigoto, *kawaii*. The tribes belonging to this second group, form a solid mass in southern Venezuela and on the upper Rio Branco, and are more or less surrounded on the north and east by the first group. Turning to the more isolated Cariban languages, we find the Apiaka of the lower Tocantins using the form *tame*, *tawe;* of the Parnahyba tribes, the Palmella have a similar form *tama* although the Pimen-

[1] *Op. cit.*, p. 135.

teira word *tciaming* may be of different origin. Of the upper Shingu tribes, the Bakairi use the form *tawe, tawi* while the Nahuqua have *teninya*. The eastern Colombian tribes used a form close to the general stem, viz., Carijona, *tamouinto;* Hianakoto-Umaua, *tame;* as did also the Carib tribes of the Lesser Antilles, *i-taman-le*.

There would appear to be little doubt but that all of these forms, with the exception of the Pimenteiro and Nahuqua, are derived from the same stem *tama* or *tawe* of which the latter, if Bakairi is established as the most archaic dialect, would be the original form. The affiliation of the *kauwai* forms with this presents some little difficulty since a shift of initial *t* to *k* seems hardly yet established in the Cariban languages.

Outside the Cariban stock, forms comparable to either the *tama* or *kauwai* stems seem not to occur, so that as in the case of the Tupi-Guarani their distribution is confined to the tribes belonging to the stock. Thus the same argument applies here, as in the Tupi-Guarani, namely that, in spite of an obvious similarity with the Negro *taba, tama, tawa* stems from which Professor Wiener would derive them,[1] the facts of distribution make such an origin well nigh impossible.

The tobacco words in use by the various Arawakan[2] tribes offer perhaps the most interesting and puzzling problems of any. Of very wide distribution, the main body of the stock extends in an irregular but practically continuous band from northern Bolivia to the Orinoco, while other isolated groups were found from the upper Paraguay to the west coast of Florida.

The majority of the tribes in the main Arawakan area use words for tobacco derived from one of two stems. The first stem is found in a compact, continuous area in the region of the upper Orinoco and Rio Negro in eastern Colombia and southern Venezuela, viz., Siusi, *(n)dzema;* Katapolitana, *dzema;* Karutana, *ndzema;* Caruzana, *zhema;* Cavere, *(scema);* Uarekina, *däma;* Guipunavi *dema;* Mandahuaca, *dehena;* Yavitero, *shama, dyäma;* Baniva,

[1] *Op. cit.*, p. 140.

[2] I am indebted to Dr. W. C. Farabee for information on variants from some Arawak as well as other S. American tribes.

djema; Piapoco, *tsema;* Maipure, *hema;* Tariana, *yäma*. To this group are probably related the forms in use by the Arawakan tribes of the interior of British Guiana, viz.: Wapisiana, *suma;* Atarois, *suma*, (*schama*); Taruma, *tuma*. The other stem, in contrast to the one just considered, is only found widely scattered, viz.: Yaulapiti (in the Upper Schingu region) *airi;* Pammari (on the Upper Purus) *hädyiri* (Cf. *odyi*, smoke); Piro (on the Ucayali) *iri;* Baniva (upper tributaries of the Rio Negro) *eri*, *äli;* Baré (ditto) *hari*, *ari*, *ali;* Goajiro (in the Goajiro Peninsula, Northern Colombia) *yüri*, *yülli;* Arawak (Guiana coast) *yaari*, *yeury*, *yulli;* Arawak (Lesser Antilles) (*ioulli*).

Unlike the first stem, which seems to have no close analogues, these forms at once raise the question of possible extensive borrowing and transmission. It may be noted first, that the Arawakan tribe of the Anti or Campa (who were in the Apurimac and Urubamba valleys north of Cuzco, and close neighbors of the Piros as well as the Quechuas) used for tobacco a form given variously as *sairi*, *seri*, *tzeri*, *leri* of which all but the last are very evidently the regular Quechua word. The neighboring Piros had, as already noted, the form *iri*, and five small, supposedly independent stocks located in this same vicinity or near Arawakan tribes using this stem, had words for tobacco which show a rather striking similarity, viz., Chapacuran, *aiwi*, *ivi*, *eve;* Ypurinan, *awiri;* Salivan, *arre;* Uitotoan, *yera;* and (?) Chavantean *waari*, *wali*, *wani*. Too little has as yet been done in the way of comparative Arawakan studies to enable one to do more than suggest that there seems to be a possibility here, of the transmission of a single stem to unrelated tribes over very extensive areas, although further analysis may show the resemblances to be in large part misleading.

The Moxos form *sabare* (*sahua*) may (?) be allied to the *sema*, *suma* series; the adjacent Baures employ a different word *sini*, which seems to find analogies in the Yammamadi *sina*. The Paressi *azie* and Saraveka *atce* stand more by themselves, as does the word in the other Matto Grosso languages of the stock in this vicinity, viz., Mehinaku, Kustenau, Waura, *höka*. The Guaná form *tcahi* is almost identical with the neighboring Otuquian form *tcaha*.

The Arawakan languages are believed by Professor Wiener[1] to have borrowed their words for tobacco from the Malinke *dyamba* meaning tobacco, and the Mandingo *duli* meaning smoke; from the former he derives the *dzema*, *sema*, *yema* series, from the latter the *eri*, *yeury*, *iouli* series. The resemblances are, it must be said, not very striking, and it requires only slight further consideration to reveal serious difficulties. In the case of the *dzema*, *sema*, *yema* series although the fact of the limitation of the supposedly introduced form to tribes of the Arawakan stock is not, because of the smaller area and more compact character of the territory in which the stem occurs, as insuperable an obstacle as in several previous instances, it is nevertheless a serious difficulty. For it is not easy to see why or how the Negro *dyamba*, for whose existence in the Antilles no evidence is given, could have spread thence or been carried by "the Negroes and Indians of Hispaniola" to the tribes of the upper Orinoco and Rio Negro, over the heads of the whole series of coastal tribes, among whom, from the same source and by the same means, a wholly different stem is supposed to have been simultaneously introduced.

Similar difficulties are met with in the case of the words derived, according to the African theory, from the Mandingo *duli*. Transmission of the stem *yuli* along the Caribbean coast from the Antilles to the Goajiro peninsula might indeed occur, but how account for the break of some six or seven hundred miles between the area of this coastal distribution and the Rio Negro region where the first of the interior tribes using the stem is found; and for the similar gaps of 500 to 1000 miles which lie between the other isolated areas in which it occurs? The assumption that these words are derived from a foreign stem introduced into the Antilles at the time of the discovery, and spreading thence for thousands of miles, in such a way as to occur only in small isolated and widely separated areas, is on its face impossible. The observed facts of distribution can only be explained on the basis of the shattering and wide dispersion of a group of Arawakan tribes having this stem in common, and who carried it with them to their final scat-

[1] *Op. cit.*, p. 154–5.

tered seats. And just such a disruption and subsequent wide migration of the Arawakan stock is generally admitted to have taken place as the result of the northern movement of the Carib peoples. In the course of these movements, other tribes *might* perhaps have borrowed the Arawak word, and thus its seeming presence in the Salivan, Ypurinan, Chapacuran, and Chavantean stocks be accounted for.

The real problem, however, is that of the Quechua-Aymara *sairi*. If the series, already pointed out, be arranged schematically with reference to geographical position, we get the following:

Quechua-Aimara-Anti
SAIRI
Goajiro *yüri* Arawak *yaari, yeury*
Salivan *arre*
Uitoto *yera* Pammari *hädyiri*
Piro *iri*—Ypurinan *awiri*
Chapacuran *aiwi, ivi*
Yaulapiti *airi*—Chavantean *waari*

Perhaps it is only a curious coincidence that the *sairi* form adjoins the *yuri, iri, airi* series, and the resemblance in sound may be purely fortuitous. Yet the similarity is striking enough for a hypothesis to be defensible to the effect that *sairi* was originally derived from the Arawakan stem. Should this connection be admitted, however, it would be further and conclusive evidence of the impossibility of the Arawakan stem being of foreign origin and introduced as a result of European contact in the early sixteenth century. For as the word *sairi* was recorded as early as 1586 in Peru,[1] and as Garcillasso de la Vega probably gained his knowledge of the term before he left Peru shortly after the middle of the sixteenth century, there can be little doubt but that the word was in use in the Quechua language at least as early as 1550. Since, however, there was no effective Spanish penetration of the interior of Venezuela much before 1530 it would have been quite impossible for the supposed African forms to have accomplished the journey of several thousands of miles across the forests of the upper Amazon,

[1] Holguin: *Vocabulario de la Lengua general de todo el Peru llamada Quechua.* Lima, 1586.

in the decade or two before the appearance of the word *sairi* in Peru.

The remainder of the South American stocks may best be considered in two groups, (1) those of the Brazilian highlands and (2) the broad belt of small stocks lying just east of the Andes and extending from the Chaco region almost to the Caribbean.

The dominant factor in the first group is the Ges or Tapuyan stock which occupied the larger part of the whole area and appears to represent the oldest stratum of the aboriginal population. The languages in use by the tribes composing this group are pretty widely variant, and this is reflected in their words for tobacco. Some resemblances, however, may be seen. Thus we have Botocudo, *hinkum*, *gninnang*, etc.; Masacara, *hina*, *hingza;* Camacan, *hiah;* Apinages, *karenio*, *kariniako;* Cayapo, *karingu*, *kalinu*, *arena;* Aponegicren, *boraho;* Caraho, *paro;* Coroado, *boke;* Puris, *poke*. The forms in use by the Botocudo group may be compared with the neighboring Chavantean (Cherontes) *kwanyeu*, *waniyeu;* the Aponegicren-Caraho with the Caririan *poiuh*, *paewi*. The Acroamirim *wari* seems to fall in with the Chavante *waari* in the Arawakan series. That there is any relation between the Arawakan Baures and Yammamadi *sini*, *sina* and the Masacara *hina* seems doubtful. The Tapuyan tribes were with few exceptions non-agricultural, nomadic tribes, and many were said not to have had tobacco when first known to Europeans. It is to be expected, therefore, that the terms for tobacco in use should show similarities to those of some of their more civilized neighbors.

The other stocks in this eastern Brazilian area appear to have had, for the most part, quite independent words for tobacco. Thus we find Trumai, *fi;* Goyatacan, *aptcign;* Bororoan, *mäh;* Carayan, *kuti;* Guatoan, (*ma*)*bo*. The last, and what is said to have been the "old word" among the Caraya, *biuwa*, may be connected with the Caririan *poiuh*, *paewi*. This may have been brought in by the Cariri from the west, since we find Apolistan *poi* for cigar. In view of the almost complete encirclement in historic times of the whole area of the Brazilian Highlands by tribes of the Tupi-Guarani stock, it is striking that there seems to be no trace among the Highland tribes of any *petun* forms.

In considering the patchwork of small stocks east of the Andes, we may most conveniently begin in the south and work northward toward the Caribbean. The Guycuruan stock of the Chaco at once affords a puzzle. The words here for tobacco are: Mocovi, *asareh, eserike;* Toba, *asiedeh, ecierok.* The striking resemblance of the Mocovi at least to the Quechua-Aymara *sairi* is at once apparent, and raises the question whether the Guycuruan languages can be added to the long list of those which appear to have derived their tobacco words from some common source. The stock was, in historic times, separated from the Quechua-Aymara by the territory of the Calchaquian tribes, of whose language we unfortunately know practically nothing. The closeness in form of the Guycuruan words to the Quechua and its location so far beyond any known Arawakan influence, suggest that perhaps the Quechuan form is the real source for both the Guycuruan and some of the other non-Arawakan terms, and that the two series of derivatives have accidentally met.

The Yurucare, Mosatena, and Tacana although long in close contact with the Quechua-Aymara and showing a considerable number of loan words from these languages in their own speech, give little suggestion of borrowing so far as regards their words for tobacco. These are in order, *kore, kos,* and *umasa, umaxa,* and all appear to be independent except for a possible (?) connection of the latter form with the Panoan *rumue.* The Chapacuran languages situated somewhat farther eastward, have Chapacura, *eve, ivi;* Pawumwa, *aiwi;* Itenes, *yove,* which, as already pointed out, may be connected with the Arawakan *iri, airi, yuri* series. The large Panoan stock shows at least two words for tobacco, Conibo, *rumue* (*dromba*?); Caripuna, *rumoe;* Sipibo, *cika, tcika.* The Conibo-Caripuna form suggests the Chibchan (Paez) *mue-hi,* while the Sipibo word may be connected with the Chibchan *hoska.* Although at present the Panoan tribes are not in contact with the Chibchan stock, they are supposed to have formerly occupied territory much farther north, where contact would not have been impossible.

The Jurian *jiya* (phonetically *'hiya?*) shows no notable resem-

blance to any neighboring forms, and this is also the case with the Mainan *pinterlo*, although the other word given from this stock, *uhualek*, might suggest connection with the Zaparan *xwaneka*. The Uitotoan *yera*, *yära iera* would appear to belong, as previously stated, to the Arawakan *yuri*, *iri* series. Jivaran *tsalano*, *sango* and the wide-spread Betoyan form *meno* seem all to be independent. Some interest attaches to a small group of four supposedly independent stocks lying between the Rio Negro and the Orinoco. The words used for tobacco by them are as follows: Makuan, *hot*, *hE(b)*; Piaroan, *hate*, *hahetue;* Puinavian, *jeup*, *job* (phonetically *höp*, *hob*); Guahiban, *joo*, *ho*. With these possibly the Guaraunan forms *aha*, *aoha*, *akae* may also be compared. The similarity between these various tobacco words suggests that these stocks, now somewhat separated, may formerly have lived in close association, and have been scattered as a result of the Cariban invasion. The Otomacan *gui* (phonetically *wi*) seems to show no decided resemblance to other neighboring forms, which is also the case with the Yaruran *gambi*.

There remains to consider two special cases in which we know, or can be practically certain, that the introduction of tobacco dates from after the period of the discovery. The first is that of the Tsonekan or Tehuelche stock of Patagonia. Here the evidence, both historical and archaeological shows that tobacco was not in use prior to the period of Spanish contact. The word in use among these tribes is given variously as *hiatca* and *golkul*. Neither of these forms shows resemblance to the neighboring terms, the Guycuruan variant *outcete* and the Otuquian *tcaha* being too remote to be of any probable significance.

The second case is that of the Alaskan Eskimo. Here, as we know from the form of the pipe, the knowledge and use of tobacco was introduced from the Chukchi of northeastern Siberia, who in their turn got tobacco and its name from the Russians. The word in use among all the Western Eskimo tribes is practically identical, viz., Kopagmiut, *tawarak;* Nuwukmiut, *tauwak;* Kotzebue Sound, *tauwak;* Malemiut, *tabak;* Ugalakmiut, *tawaku* and is obviously a close imitation of the Čhukchi *tawax* (*tawar*, *taak*). The

Eskimo of Labrador, on the other hand, who probably secured tobacco first from the eastern Algonkian tribes, use a quite different form, *tupiving:* In the case of the western Eskimo thus, where we know the word to have been introduced, we have a very close approximation to the form tobacco, and no evidence of any greatly mutilated or modified forms as in the cases claimed in other areas by Professor Wiener.

The foregoing rapid survey and comparison of the words for tobacco in the majority of American Indian languages seems to establish the following results. Speaking in general there are approximately as many distinct stems for the word "tobacco" as there are separate linguistic stocks, and as a rule each stock has its own characteristic stem or stems. In some cases the same stem appears to be in use by two or more different stocks, but this is on the whole a rare phenomenon. Only one case has been found in which a single stem seems to have a wide distribution among unrelated languages, that of *sairi*, for which, however, no extra-American source can be claimed. The situation is, in fact, just what would be expected if tobacco had been known and used by the American Indians for centuries or even thousands of years, and tobacco words seem to be quite on a par with other words relating to native plants and animals.

Where more than one stem is in use by a stock, it is probable that one form is really characteristic, while the other or others are borrowed from neighboring stocks by peripheral tribes; or the stems for "tobacco," "to smoke," or "pipe" are used alternatively by different portions of the stock. Where similarities between the words for tobacco suggest borrowing, in almost every case the tribes concerned either now are or at one time may have been in contact.

The facts brought out tend very strongly to disprove, on purely linguistic grounds, the theory recently advanced by Professor Wiener, that tobacco and its use as well as many of the names for the plant, were of European and Negro introduction at the period of the discovery. The number and variety alone of the stems in use in the New World would negative such an hypothesis, in spite

of its author's nimbleness in deriving Indian forms now from one, now from another Negro word. The facts of the distribution of the supposed Negro forms, however, are such as to preclude the origin of the American forms from foreign stems, for the restriction of particular stems to particular stocks rather than their indiscriminate dissemination, and the existence of these typical stems in far distant and isolated tribes belonging to the stock, make any explanation, other than one based on expansion and migration of an originally united group of related tribes, practically impossible. Lastly, further evidence of the native origin of many of these tobacco words, is seen in the existence of other words in the language, formed from related stems, a condition which could hardly exist were the tobacco words of extraneous origin.

That similarities can be found between certain Negro languages and some in America is obvious. They may be found between any two languages in any part of the world. The Mande *tama*, *tawa*, *taba* unquestionably suggests the Micmac *tumawa* or the Cariban *tamo;* but in view of all the known facts it is as unjustifiable to declare these American words to be derived from the Mande, as it would be to claim that the Cherokee *tcahu* was the source of the Otuquian *tcahi*, *tcaha;* the Miwok *kasu* of the Mosatena *kos;* the Creek *hitci* of the Saravekan *hatce;* or that because the Maori of New Zealand call the sun *ra*, they must have borrowed the term from Egyptian!

As stated in the beginning, the fact that tobacco and its use was known in America, centuries before the earliest European contact is abundantly proved by archaeological data; that it was in use at the time of the discovery is shown by historical evidence. To have made the foregoing investigation of the words for tobacco, with a view to showing their native rather than foreign origin, has been consequently in part, a work of supererogation. It has, however, disclosed a number of interesting problems and suggested several unsuspected possibilities in the way of cultural influences. Our knowledge of the great majority of American Indian languages is as yet too incomplete to enable us to make trustworthy analyses of much of their vocabulary, and for many lan-

guages must always remain so. But with increasing knowledge it may sometime be possible to obtain by some such study as has here been tentatively made, real clues as to the sources and lines of transmission of many of the cultivated plants of the New World.

HARVARD UNIVERSITY

NOTES ON THE STONE AGE PEOPLE OF JAPAN

By H. MATSUMOTO

I. Introduction

PALEANTHROPOLOGICAL and archaeological studies in Japan have made great advances in the last few years, owing to our successive discoveries of sites yielding many good human skeletons. Unfortunately, the majority of the reports of many Japanese authors were written in Japanese, so that they are generally not available to the authorities of foreign countries. I have been asked, first by Prof. M. Courant and now again by Prof. W. K. Gregory, concerning this subject. I am, at present, a visitor in America and have no Japanese books or papers of reference at hand, so it is impossible for me to make up a precise report with accurate statistical tables. It should be remembered that these notes have been written only from memory.

II. More Important Stone Age Sites Yielding Human Skeletons

1. *Kitchen-midden of Aoshima*, Tome district, Province of Rikuzen, northeastern part of the main island. This site, discovered by me, consists of two shell-bearing beds; the upper one is rather poor in shell fragments, but the lower one very rich in this material. The shells are mostly from fresh water and only partly marine. I once obtained fourteen human skeletons from the upper bed of this site, and I am going to proceed with further researches after my return home. The stone implements and pottery of this site are of the type of the earlier stone age of Japan.

2. *Kitchen-midden of Miyato island*, one of the islands of Matsushima, also Province of Rikuzen. The presence of human skeletons at this site was discovered by me and my coöperator, Dr. I. Hayasaka of our institute. This site now belongs to our institute as a ground for our study. It consists of about eighteen

shell-bearing beds and ranges vertically more than twenty feet. I have obtained many good human skeletons from this site, and it is certain that it contains many skeletons as yet untouched. The stone implements and pottery of this site are of the type of the mediaeval stone age of Japan.

3. *Kitchen-midden of Tsukumo*, Asaguchi district, Province of Bitchû, western part of the main island. The first discovery of human skeletons at this site was made by a landlord of this locality, Mr. S. Matsuyeda, by whom I was given a number of human skeletons. Since my preliminary short report of the human skeletons of this site, Profs. Hamada, Suzuki, Kyiono, Ôgushi, and Hasebe have undertaken the study of the same subject. This site is a shell-bearing bed of the mediaeval stone age of Japan, covered over by a layer of the earlier metal age. The bed yielding human skeletons is undoubtedly the former.

4. *Site of Kô*, Province of Kawachi, western middle part of the main island. The first discovery of human skeletons from this site was by Prof. Hamada. He, as well as Professors Ôgushi, Koganei, and Hasebe, are now studying on this site, which is not a shell-heap. The lower layer yields human skeletons, associated with stone implements and pottery of the latest mediaeval stone age, and the upper layer yields pottery of the earlier metal age.

5. *Kitchen-midden of the Cave of Ôsakai*, Himi district, Province of Kaga, northern coast of middle part of the main island. This site, discovered by Mr. J. Shibata, consists of six shell-bearing beds, the lowest bed of which is of the mediaeval stone age, the next two beds of the later stone age, and the upper three beds of the metal age. The second and the fourth beds from below yielded a small number of human skeletons.

6. *Kitchen-midden of Higashi-ataka*, near Kumamoto, Province of Higo, Kiushû. This site has yielded many human skeletons which are, at present, being studied by Prof. Yamazaki. The pottery of this site is said to be of the types of both the mediaeval and the later stone age of Japan. It is not yet clearly known which of the two types of pottery accompanied the human skeletons.

7. *Kitchen-midden of Todoroki*, near Kumamoto, Province of

Higo also. A part of the human skeletons found at this site has been reported upon by Prof. Suzuki, while another part is just being studied by Prof. Yamazaki. I am not informed concerning the pottery of this site, but it may possibly be of the type of either the mediaeval or the later stone age of Japan.

Several other sites known to yield human skeletons are not yet carefully excavated and thoroughly studied.

III. Chronological Subdivisions of the Stone Age of Japan—Degenerative Evolution of the Decorative Pattern of Pottery

All the stone age sites hitherto discovered in Japan belong to the neolithic age, or new stone age, in the European classification. Nevertheless, the divergencies observed to exist among several sites and among several types of remains are very great, suggesting that the stone age of Japan might have had a considerable duration.

Among the neolithic remains, the pottery is one of the most valuable horizon-indicators. Tracing the changes or evolution of the pottery from below upwards in any site, we can clearly recognize the direction of changes or evolution which have taken place in the stone age pottery. According to my own statistical studies of the pottery collected by careful, serial excavations, the following important changes have been proved occurring in the stone age pottery of Japan:

1. Richer in large bottomed pottery below, and richer in small bottomed pottery above.

2. Richer in thick pottery below, and richer in thin pottery above.

3. Richer in pottery with coarse and rough mat impression below, and richer in that with fine and nice mat impression above.

4. Richer in pottery with ornamental pattern of lower order below, and richer in that with ornamental pattern of higher order above. As to the ornamental pattern of the same order, richer in the more well-developed ornamental pattern below, and richer in the more upwardly retired ornamental pattern above.

5. Richer in reddish or brownish pottery in the earlier than in

the mediaeval stone age, and richer in very dark pottery in the mediaeval than in the earlier stone age; and again, richer in very dark pottery in the mediaeval than in the later stone age, and richer in reddish or yellowish pottery in the later than in the mediaeval stone age.

Now I have to discuss the orders of the decorative pattern of pottery. They are:

1. Bas-relief decorative pattern of curve and spiral design. This decorative pattern is observed in the earliest pottery of Japan. It consists of wide bas-relief ribbons of curve and spiral design

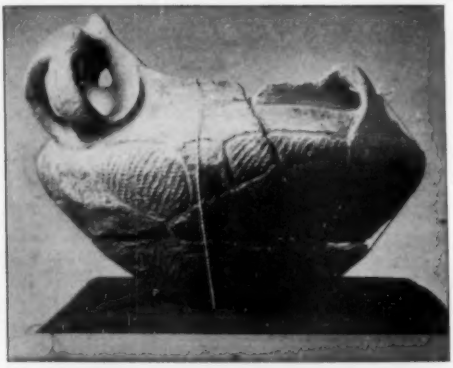

FIG. 11.—Type of pottery of the lower earlier stone age.

applied on the outer surface of pottery. A small number of very elaborate handles also of bas-relief and spiral design developed on the oral margin of pottery, as a part or parts of this decoration. This decoration usually coexisted with mat impression on the outer surface of the same pottery.

This decoration progressed upwards, toward the oral margin of pottery, uniting at last with the original oral edge so as to form a double edge. Hand in hand with the retirement of this decoration, the elaborate handles became smaller and simpler also.

2. Incised decorative pattern of curve and spiral design. This decorative pattern was of the second order, developing upwards,

just below the retiring bas-relief decoration of the first order. The decoration of this second order was sometimes bas-relief consisting of narrow ribbons especially in its earlier stage, but most commonly it was incised throughout. This decoration might sometimes stand alone by itself, but usually coexisted with mat impression on the outer surface.

This decoration reached its highest tide, when the decoration of the first order had just retired to the very margin of the mouth of pottery. At its highest tide, it occupied almost the whole outer

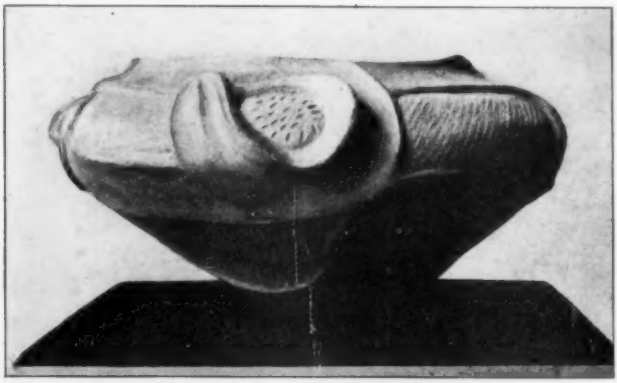

FIG. 12.—Type of pottery of the lower earlier stone age.

surface of the vessel or of the body of it, in the vessels without or with differentiated neck, respectively. After its highest tide, it again retired upwards, toward the oral margin or the lower border of the neck, respectively, in the pottery without or with differentiated neck. In its retiring stages, its area was bordered both above and below by one or a few incised streaks encircling the pottery. As a result of its extreme retirement, there was left only one or a few horizontal streaks around the vessels just below the oral margin or at the upper half of its shoulder. This decorative pattern of the second order thus converging into horizontal streaks, may be looked upon as the decorative pattern of the primary order of geometrical design.

3. Decorative pattern of free mat impression. The mat impression in general was one of the earliest characters of the stone age pottery. Very often it was included in the area occupied by the decorative pattern of curve and spiral design, while in other cases it was exclusive and free from that area. The free mat impression to be discussed here is, of course, that of the latter cases. Such a free mat impression was the decoration of the third order.

As a typical case, this decoration arose and developed upwards, just below the lower border of the retiring decorative pattern of the second order. In another case, it took the same course just below the retiring decorative pattern of the first order. At its highest tide, it occupied almost the whole outer surface of the

FIG. 13.—Type of pottery of the upper earlier stone age.

vessel or of the body of it. After its highest tide it had again just the same destiny as its forerunners.

In its earlier stage the mat impression in general was coarse and rough; but afterwards it became finer and nicer. The free mat impression of the later stage assumed very often a repetition

of pinnate arrangement. Thus the decorative pattern of the fine, pinnately arranged, free mat impression prevailed in the later mediaeval stone age of Japan.

4. Decorative pattern of the secondary order of geometrical design. As a typical case, a few or many incised parallel streaks arose encircling the vessel just below the retiring free mat impression. These stripes correspond to the decoration of the fourth order in general, and that of the secondary order of geometrical design.

FIG. 14.—Type of pottery of the upper earlier stone age.

This decoration developed upwards hand in hand with the retirement of the free mat impression. When the free mat impression vanished as a result of its extreme retirement, both the primary and secondary decorative patterns of geometrical design became united, forming a compound decorative pattern of geometrical design. In another instance, the decorative pattern of the third order in general persisted as a mat impression itself, or as its modification, an incised false mat impression; then, both the primary and secondary decorative patterns of geometrical design and the mat impression or false mat impression formed together a compound decorative pattern of geometrical design.

The degenerating handles of the vessels belonging to the decorative pattern of the first order persisted until the last of the curve

and spiral design; then they disappeared almost entirely. They coexisted no longer, as a rule, with the well-established decorative pattern of geometrical design, except in the stone age pottery of Riu-kiu.

It may be true that certain elements of the geometrical decorative pattern were imported into Japan from the continent; but I cannot agree by any means with those authors who declare that the whole geometrical decorative pattern was so derived. In my opinion, the change of the decorative pattern of the stone age pottery of Japan from curve and spiral design to geometrical design was chiefly an evolution but not a revolution. Consequently, we are obliged to look upon the artifacts of the stone age of Japan

FIG. 15.—Type of pottery of the middle mediaeval stone age.

as those made by the genuine ancestors of a greater part of us modern Japanese. This view will be confirmed again by a study of the racial types of both the stone age and the modern Japanese.

Looking over all these changes of the stone age pottery, it may safely be said that they were chiefly degenerative in the limitation of the decorative pattern. It appears that the stone age pottery changed or evolved according to the law of the economy of labor and time. Again, the succession of the various orders of decorative pattern corresponds well to Dollo's law as is well known in our palaeontology.

In accordance with these facts and considerations, I subdivide the stone age of Japan chronologically as follows:

1. *Earlier stone age*, or period of bas-relief pattern of curve and spiral design.

Pottery large and very thick; mat impression, very common, coarse, and rough; bas-relief, decorative pattern of curve and

FIG. 16.—Human figure of the middle mediaeval stone age.

spiral design very common; handles, very large and elaborate (figs. 11 and 12).

(1) Lower earlier stone age:—bas-relief pattern of the first order very well developed.

(2) Upper earlier stone age:—bas-relief pattern of the first order usually limited to the upper part of the vessel (figs. 13 and 14).

2. *Mediaeval stone age*, or period of incised pattern of curve and spiral design.

Pottery, moderately thick to thin; mat impression, very common, coarse, and rough to fine and nice; bas-relief patterns very few; incised decorative patterns of curve and spiral design, common; handles, large to very small.

FIG. 17.—Type of pottery of the upper mediaeval stone age.

(1) Lower mediaeval stone age:—pottery, moderately thick; mat impression, coarse and rough; incised patterns of curve and spiral design, very common, very well-developed; handles, large and elaborate.

(2) Middle mediaeval stone age:—pottery, thin; mat impression, fine and nice; incised pattern of curved and spiral design, very common, very often limited to the upper part of the vessel; handles, small and simple (figs. 15 and 16).

(3) Upper mediaeval stone age:—pottery, thin; mat impression, fine and very nice; free mat impression decoration, very well-developed, very often assuming a repetition of pinnate arrangement; incised decorative pattern of curve and spiral design, persisting but rather less common, mostly limited to the upper part of the vessels; handles, small and simple, or entirely absent (fig. 17).

3. *Later stone age*, or period of incised patterns of geometrical design. Pottery, thin; mat impression, less common to entirely absent, besides incised false mat impression; bas-relief decorative patterns, very few; decorative patterns of curve and spiral design, entirely absent; that of geometrical design, common; handles, almost entirely absent, those of the secondary order, different from that of the handles of the preceding ages, might sometimes be present.

(1) Lower later stone age:—mat impression decoration, persisting, usually limited to the upper part of the vessels; incised false mat impression, present.

(2) Upper later stone age:—mat impression decoration, entirely

FIG. 18.—Type of pottery of the Hanibe-Iwaibe period.

absent; incised false mat impression, rather common. The later stone age was followed by the Hanibe-Iwaibe period which belongs to the metal age.

4. *Hanibe-Iwaibe period*, or earlier metal age. Coexistence of the Hanibe pottery, which is very similar to the pottery of the upper later stone age, and the Iwaibe pottery, which is a grayish or dark bluish hard pottery and resembles very much that of the ancient Koreans (figs. 18 and 19).

This period corresponds to the protohistorical and earlier historical ages of Japan. The chronological succession was very gradual in western Japan, while it was interrupted by the absence of the later stone age culture in northeastern Japan where the mediaeval

FIG. 19.—Type of pottery of the Hanibe-Iwaibe period.

stone age culture was followed immediately by the Hanibe-Iwaibe culture. That is, the changes of the culture were an evolution throughout in western Japan, but partly an evolution and partly a revolution in northeastern Japan.

IV. BURIAL CUSTOMS OF THE STONE AGE PEOPLE

The stone age burials of Japan were almost always contracted, as clearly observed by Mr. Uchida, in the site of Tsukumo; by Prof. Suzuki, in the site of Higashi-ataka; by Prof. Hamada and Mr. Torii, in the site of Kô; by Prof. Ôgushi, in the sites of Kô and Tsukumo; by Prof. Kiyono, in the site of Tsukumo; by myself, in the sites of Miyato island, Aoshima, and Kô; by Prof. Koganei and Mr. Shibata, in the site of Kô; by Prof. Hasebe, in the sites of Kô and Tsukumo; and so on; but very rerely extended, as observed by Prof. Kiyono and Ôgushi in the site of Tsukumo.

The bodies, skeletons when we discover them, were laid in tombs either with the back directly down or slightly to one side. The burials on the back appear to be more common in the sites of western Japan, as Tsukumo, Kô, and Higashi-ataka, and in the earlier stone age sites of northeastern Japan, as Aoshima and

certain less important sites (fig. 20); the tilting toward one side appears to be more common in the later sites of northeastern Japan, as Miyato island (fig. 21). In these, the tilting toward the right side was much more common than that toward the left.

The direction of the long axis of the body was exceedingly variable in the sites of northeastern Japan. In the site of Aoshima

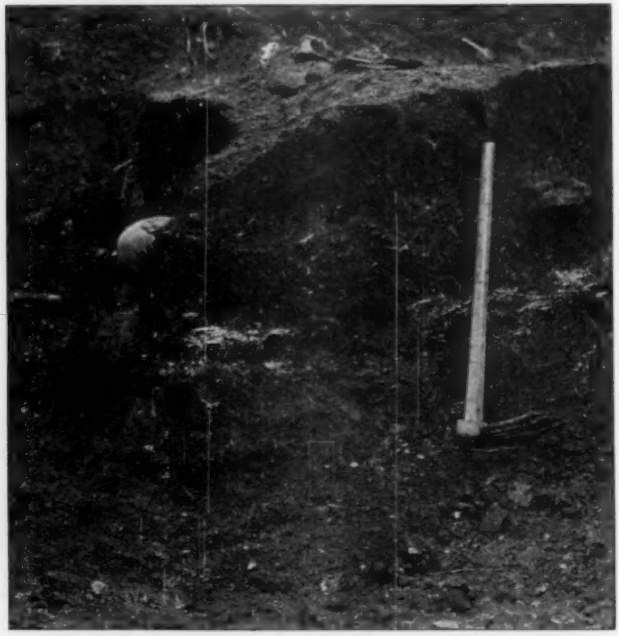

FIG. 20.—Burial of the Aoshima type, first stage of the first period.

the head, not the face, of the skeleton was directed north or east or south but never west; in the site of Miyato island, southeast (most common direction in this site) or northeast or northwest but never southwest. It is reported by Profs. Ôgushi and Hasebe that the northeast and southeast directions were most common in the sites of Tsukumo and Kô respectively.

Double burials have been observed to be present very rarely. I once discovered a double burial consisting of a very aged male and a child in the site of Miyato island. The aged man was

FIG. 21.—Miyato island burial.

laid on his right side, assuming an attitude embracing the child who was laid on his back and left side. Mr. Matsuyeda has also discovered a double burial consisting of two adults in the site of Tsukumo. I do not know the sex of these two adults; but, judging

from the photograph presented by Mr. Matsuyeda, one or both of these adults might be that of a male. Both of them were laid on their backs.

Two female skeletons from the site of Aoshima were found with ear ornaments of clay, one on each ear. These ear ornaments are dumb-bell shaped, the anterior and posterior halves being almost of equal size and shape. This type of ear ornament is a common characteristic of the sites of western Japan and the earlier sites of northeastern Japan.

Skeletons of one young female and of a child at the site of Miyato island were found with stone beads as ear ornaments,—one for each ear. The beads belonging to the young woman were made of jade and are charmingly executed. The jade might have been imported into Japan from the continent at such an ancient time. Again, the child skeleton of the double burial mentioned above was found with eight stone beads on the position corresponding to his right ear. Though not attached to any skeleton, another type of ear ornament of clay was obtained from this site. It is rather cup-shaped, the anterior half being much larger than the posterior. This type of ear ornament is characteristic of the later sites of northeastern Japan.

It has also been reported by Profs. Ôgushi, Koganei, and Hasebe, that several skeletons from the site of Kô were found bearing dumb-bell shaped ear ornaments of clay or stone ear ornaments shaped like an incomplete ring; and by Prof. Ogushi that one skeleton from the site of Tsukumo was found bearing ear ornaments made of deer's antler and shaped like an incomplete quadrangular ring. Prof. Ôgushi considers that the ear ornaments made of deer's antler and shaped like an incomplete ring might be prototypes of the stone ear ornaments of similar shape, while Prof. Hamada looks upon the latter as prototypes of the metal ear rings of the earlier metal age of Japan.

One very stout male skeleton from the site of Miyato island was discovered bearing two compact bands consisting of some eighty beads made of bird's bones, around his wrist, and just in front of his wrist five very nice and elegant ornaments, one of which

was made of deer's antler and four of boar's tusks (the deer was a large variety of the sika, and the boar was a very gigantic race or species and is now everywhere extinct).

It has also been reported by Profs. Kiyono, Ôgushi, and Hasebe that some skeletons from the sites of Tsukumo and Kô were found bearing an elegant ornament made of deer's antler, just in front of the wrist.

The majority of the skeletons from the site of Miyato island were found bearing the red tint of iron oxide on the upper portions of their bodies, especially on the face and breast. It might have been thrown over or tinted over the dead body by the mourning relatives and intimates. I have witnessed the fact that the child and young female skeletons were especially rich in this red tint.

One skull from the site of the cave of Ôsakai was reported by Mr. Shibata to have been found tinted all over the face with red color. In the other sites, such as Aoshima, Tsukumo, Kô, Higashi-ataka and Todoroki, no skeleton with red paint was ever discovered.

I have learned from Dr. Krischtofowitsch that a quite analogous burial of skeletons bearing red color was once observed in a certain kulgan near Odessa, excavated by the Geological Institute of the University of Odessa. It may also be analogous to what has been observed in certain Indian burials.

Several skeletons from the site of Miyato island were found lying on a layer of ashes and cinders. This layer of ashes and cinders might have been purposely prepared in the grave for the reception of the dead. In all the other important sites no skeleton was ever found lying on such a layer of ashes and cinders.

As a unique example in the site of the Miyato island, I found one large, leavy, round stone placed directly on a baby skeleton. It is reported by Profs. Ôgushi and Hasebe that such a large, heavy, round stone laid on the thoracic region of a skeleton has been found by no means rarely in the site of Kô. Certain anthropologists consider that such a stone might have been put on the dead to prevent the waking of the spirit. According to the Ainuan belief, any spirit wakened from the dead is a demon, which causes various evils to living people.

As reported by Messrs. Kasai, Motoyama, and Prof. Ôgushi, large jars containing a baby skeleton were sometimes found in certain sites including that of Tsukumo.

As I have clearly observed in the sites of Aoshima and Miyato island, the graves were very shallow. It appears that the dead were laid down in such shallow graves and covered with earth, which formed then a very low heap above the body. The vertical depth from the top of the heap to the floor of the grave measured only about a foot or less. As the grave was so shallow, the highest parts of the skull and knees were almost above the level of the original surface of the ground at the time of burial.

V. Custom of Removing or Modifying Several Teeth Artificially

The existence of such a custom in the stone age of Japan was first noticed by Prof. Koganei. Afterwards, very abundant data on this subject were added by Profs. Ogushi, Ôkiyono, Satô, Hasebe and myself.

It appears to me that this custom prevailed not at all or but little in the earlier stone age, so that no sign of the existence of such a custom has yet been discovered in the site of Aoshima; it then increased gradually, so that this custom was demonstrated only in a part of the adults from the sites of Nakazuhama, Prov. of Rikuzen, and Yoyama, Prov. of Shimôsa; it then reached its highest development in the middle to later mediaeval stone age, so that evidence of this custom was found in all the adults from the sites of Miyato island and Tsukumo; it then decreased gradually, so that it was apparent only in part of the adults from the sites of Kô and the second bed from below of the cave of Ôsakai; and finally it disappeared everywhere during the following metal age.

This custom is quite variable as to its types, which may be distinguished as follows. The typical example of the first is to remove the pair of upper canines. The modifications of this type are to remove the upper canine of one side and the second upper incisor or the first upper premolar of the other side; to remove one

of the second incisors, the canine and the first premolar of one side and two of the same of the other side of the upper jaw; and to remove twoor three of the same of either side of the upper jaw.

The typical practice of the second type is to remove all the four canines of the upper and lower jaws. The modifications of this type correspond to those of the first type in both the upper and lower jaws. In a few examples the first upper incisors were also removed.

The third type and its modifications are to remove all the lower incisors or all the lower incisors and canines; besides they correspond to the first type and its modifications in the upper jaw.

Another very unusual modification of the third type is to remove the pair of upper canines and all the lower incisors and canines and to modify artificially the natural form of all the four upper incisors. A unique example of this rare type has been obtained by Prof. Koganei from the site of Kô. This example may possibly be feminine; her first upper incisors are three-pointed with two artificial indentations and her second upper incisors two-pointed with one artificial indentation.

The first and second types and their modifications are proved to exist in the sites of eastern middle to northeastern Japan, while all the three types and their modifications are found to exist in the sites of western Japan. According to Prof. Hasebe, the third type and its modifications appear to be limited to female skeletons, in the sites of Tsukumo and Kô; but according to Prof. Ôgushi and myself there might be some exceptions to this rule.

This custom was not found at all in the child skeletons. It appears that the operation of removing several teeth had been carried on in the adolescent stage in both men and women. Such a custom of removing or modifying several teeth is reported to exist among many races with primitive culture. In the neighborhood of Japan, some of the aborigines of Formosa and the Philippines have such a custom, while the modern Ainu have no such custom at all.

VI. Skeletal Characters of the Stone Age People of Japan—Racial Types

The goodly number of skeletons hitherto found in the stone age sites of Japan all show a uniformity in certain characters, with a considerable divergency in certain other characters. Their common characters are as follows:

Common Characters. Calvarium, rather low and flattened above, being not very convex; lambda-inion curve, long and strongly curved; inion-opisthion curve, very short. *Os incae* or interparietals, often well-developed, consisting of two to four conspicuous pieces. Glabella, *eminentia supra-glabellaris* and superciliary ridges usually united together so as to form a conspicuous, rhomboidal, boss-like projection just above the root of nose. Face, shallow, very wide with jugals prominently projecting. Zygomatic arches, very deep and stout. Upper edges of orbits, almost horizontal instead of being divergent downwards. Nasals, usually narrow, strongly arched up, being strongly curved in horizontal section; but, in a few of the female skulls, they are observed to be rather flattened and not arched up so much. Ascending bar of mandible, nearly perpendicular to the horizontal bar. Dental arches curved like a half of either an ellipse or a circle, the anterior parts being smoothly curved and not angular at the corners corresponding to the canines.

Vertebrae, not very stout. Sacrum, appearing to be usually dolichohieric and curved very feebly. Ribs, extraordinarily deep and stout. All the long bones of both the upper and lower limbs are very stout, with very well-developed projections and keels for the attachment of tendons of muscles; hence it is also evident that the muscles might also be very well-developed. Metacarpals, metatarsals, and phalanges appearing to be rather slender.

Proportion of the length of fore-arm to that of upper arm, near that in the Ainu, being larger than that in the major modern Japanese. Greater tuberosity of humerus, high, arising almost to the same level as the head; deltoid process strong bicipital. Both radius and ulna curved very conspicuously; process of radius, very strong.

Femur, curved and obtorted very strongly; linea aspera, extraordinarily well-developed; third trochanter, often well-developed and sometimes attaining an enormous size. In some skeletons the upper halves of the femurs are strongly compressed anterio-posteriorly. Tibia, strongly curved, sometimes strongly compressed laterally; anterior keel, blunt, being rounded in horizontal section

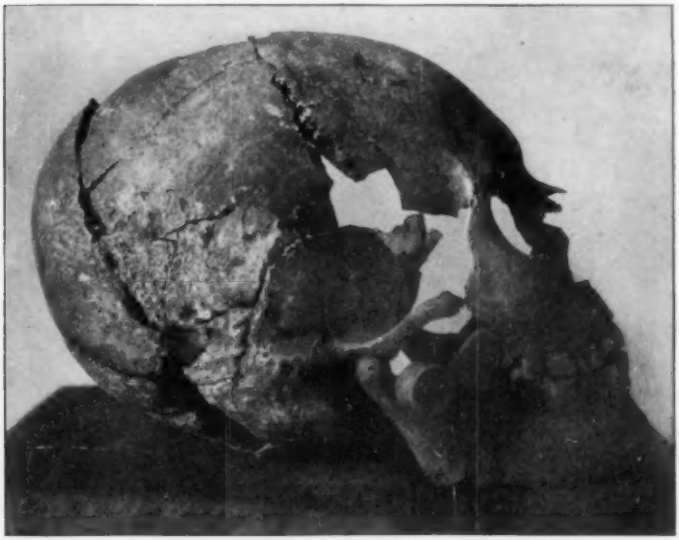

FIG. 22.—Aoshima skull, first stage of the first period.

(that in the major modern Japanese forms a sharp cutting edge). Fibula, extremely stout with a number of very well-developed vertical keels and a number of concave surfaces bordered by these keels, very often strongly compressed laterally. The longer axis of the posterior surface of calcaneum inclines inward down, and outward up, instead of being nearly vertical or just the reverse. Lower surface of foot, flat, being not very concave.

Almost all these common characters are also represented in the skeletons of the modern Ainu. Again, in many of these common

characters the stone age people of Japan and the Ainu appear to resemble the upper palaeolithic people and a certain part of the neolithic people of Europe.

Notwithstanding these common characters are represented, the skeletons from the stone age sites of Japan show a considerable divergence in the other characters. I will proceed with my description, subdividing them into a number of racial types for convenience as follows:

A. *Aoshima Type.* Skeletons from the site of Aoshima, and a part of the skeletons from the site of Miyato island. Moderately short, male adults standing about five feet two to four inches. Large-headed. Dolicho-mesocephalic. Glenoid fossae, shallow; post-glenoid spine, rather deep. Shallow-faced; though the face is not so shallow in absolute measurement, yet it is so broad as to be ranked among shallow-faced types in facial index; forehead, retired, and face, convex in lateral view, more or less strongly prognathous. Floor of the narial cavity and front surface of the upper jaw communicated together by a pair of grooves. Torus palatinus, often well-developed. Mandible, very strong; chin, not very strongly projected, and rounded in upper and lower view, except in some female skulls. Palatine and dental arches of both jaws, more or less long and large, like a half of an ellipse. Teeth, strong; molars, often of unreduced type, the upper ones being usually four-cusped and the lower ones usually five-cusped (fig. 22).

B. *Miyato Dwalf Type.* Part of the skeletons from the site of Miyato island; also a part of those from the sites of Tsukumo and Kô. Very short, male adults standing about five feet to five feet two inches. Size of head, moderate; meso-brachycephalic, and also to brachycephalic. Glenoid fossae and post-glenoid spine, moderate. Shallow-faced, the face being very shallow both in absolute measurement and in proportion; forehead, not retired; straight-faced, orthognathous.

Floor of the narial cavity and front surface of the upper jaw parted from each other by a ridge. Torus palatinus, not well-developed. Mandible, rather weak; chin, strongly projected, angular in upper and low view, quite like that of the European.

Palatine and dental arches of both jaws, short and small, like a half of a circle. Teeth, weak; molars, of reduced type, the second and third upper ones being usually three-cusped and the second and third lower ones usually four-cusped.

The dwarf type of the sites of Tsukumo and Kô, which is provisionally referred to the present type, appears to be more brachycephalic than the typical type of the site of Miyato island.

C. *Tsukumo Tall Type.* Part of the skeletons from the site of Tsukumo, and also of Kô. Tall, male adults standing about five feet six or seven inches. Large-headed, but may be moderate f taken in proportion to the height of body. Meso-brachycephalic. Glenoid fossae, deep; post-glenoid spine, very strongly reduced. Shallow-faced; forehead, not retired; straight-faced; orthognathous. Floor of the narial cavity and front surface of the upper jaw well-parted from each other by a ridge. Torus palatinus, not well-developed. Mandible, weak; chin, strongly projected, angular in upper and lower jaw, quite like that of the European. Palatine and dental arches of both jaws, short and small like a half of a circle. Teeth, very weak; molars of reduced type; the second and third upper ones being usually three-cusped and the second and third lower ones usually four-cusped. Tibia and fibula, long, both in absolute length and in proportion to the length of the femur.

The longer bones of the lower limbs of this type much resemble those of the Crô-Magnon type, though the former type is evidently much more progressive than the latter type in many cranial characters. Moreover, some broad-headed skulls of the Miyato dwarf and Tsukumo tall types resemble very strongly the skulls of the Furfoot-Grenelle type. The Miyato dwarf and the Tsukumo tall types are quite like the European in the general structure of the face and especially in that of the jaws and teeth. Indeed, they are nearer the European than the Ainu are to the same.

VII. Racial Types of the Modern Japanese and Ainu

It has become evident by the studies of Profs. Koganei and Hasebe that there are two racial types among the modern Ainu

of Hokkaido. One type, including about two-thirds of the total number of the modern Ainu of Hokkaido, comparatively tall (about five feet three to four inches or near that) and comparatively long-headed (cephalic index, ca. 75–76±), while the other type, comprising about one-third the total number, is very short (about five feet to five feet one inch) and comparatively broad-headed (cephalic index, ca. 79–80±). As far as I can judge from Mr. Torii's numerical tables, the modern Ainu of the Kurile islands appear to consist chiefly of the first type. The modern Ainu of Saghalin being also comparatively tall and long-headed, appear to me also to consist chiefly of the first type.

FIG. 23.—*Left*, modern Ainu skull; *Right*, skull of the Miyato type.

The first type of the modern Ainu appears to correspond well to the Aoshima type of the stone age, and the second type of the same to the Miyato dwarf type of the stone age (fig. 23).

Analytical studies of the racial types of the modern Japanese have been made by Prof. Hasebe and Mr. Matsumura. As a result of their analytical studies, four racial types have been recognized to exist among the modern Japanese,—Ishikawa and Okayama types by Prof. Hasebe, and Chikuzen and Satsuma types by Mr. Matsumura. The Ishikawa type is characterized by the very short stature (five feet to five feet one inch), not very broad head (cephalic index, ca. 78±), straight and shallow face and weak jaws;

the Okayama type by the tall stature (five feet five inches or more), broad head (cephalic index, ca. 82 or more) convex and deep face and strong jaws; the Chikuzen type by the tall stature (five feet five inches or more) and not very broad head (cephalic index, ca. 78±); and the Satsuma type by the very short stature (five feet one inch or near that) and broad head (cephalic index, ca. 82±).

I have been informed privately by Mr. Matsumura that both his Chikuzen and Satsuma types may be shallow-faced. The Ishikawa type is to be met with abundantly in the northern middle part and northeastern part of the main island; the Okayama type in the coastal districts around the Inland sea, in Kinai, *i.e.*, the former capital Kyôto and its vicinity, and in the western middle part of the main island; the Chikuzen type in the northern part of Kiushû; and the Satsuma type in the southern parts of both Kiushû and Shikoku.

The Ishikawa type appears nearly, though not yet thoroughly, to correspond to the Miyato dwarf type of the stone age, and the Chikuzen type also nearly to the Tsukumo tall type of the stone age. The Ishikawa and Chikuzen types may possibly be Mongolianized survivors of the Miyato dwarf and Tsukumo tall types, respectively. In the stone age already, the shorter type of the sites of Tsukumo and Kô appears to be more broad-headed than the typical Miyato dwarf type of the site of Miyato island. Then, there may be a certain probability that both these dwarf types belong to local varieties of one and the same branch which show the divergency, being more long-headed northeastwards and more broad-headed southwestwards. If this view be correct we may expect the presence of a racial type characterized by short stature and broad head in the extreme southwestern Japan. Then, the Satsuma type fits strictly to the expected racial type. The Okayama type, which has been looked upon by Prof. Hasebe himself to be the Korean type of the Mongolian stock, is not yet actually discovered from the stone age sites of Japan. This type might have invaded Japan either at the close of the stone age or at the dawn of the metal age.

VIII. Natural Position of the Stone Age People of Japan—Concluding remarks

The Ainu, nowadays, are a quite isolated and well characterized race. Such an isolated distribution cannot in any case be looked upon as a primary condition. The races of mankind are comparable to the local varieties of animals. We have learned that the local varieties, or a group of local species closely allied with each other, should primarily be ranged or distributed like a chain or a network. The isolated distribution of the Ainu, nowadays, represents only a single link of a chain or a network. Thus, our search for the presumed missing links which formerly connected the Ainu to their primary relatives of far distant lands, proceeds. Now, our search is being answered by the successive discoveries of the human skeletons of the stone age people of Japan.

Many European authorities consider that the Caucasian, Ainu, and Australian are to be grouped together in a great racial stock. And, I think they are right. In many physical characters the Ainu appear to be much more progressive than the Austrialian and a little more primitive than the European.

As already stated, the stone age people of Japan are very near the Ainu, and some of them are more closely related to the European than the Ainu due to the same being evidently more progressive than the Ainu. And it is almost evident that they are to be grouped together with the Caucasian, so far as the Ainu should be grouped together with the latter. I have come to look upon the Miyato dwarf and Tsukumo tall types as corresponding to two discoveries of the missing links of the great racial chain, namely the Aino-Caucasian. The Aoshima type, of which little altered survivors may be represented by the more typical type of the modern Ainu, might probably be very near the ancestral type of both the Miyato dwarf and the Tsukumo tall type.

The Aino-Caucasian range from Japan to Europe along the margin of the Asiatic continent, while the Mongolian occupy the central main part and the north of the same. Why has such a distribution arisen? It is obvious from the "theory of center of evolution and dispersion" developed by the eminent authorities

of the American Museum of Natural History. The Aino-Caucasian are pre-Mongolian in a certain point of view; the former corresponds to the group which had been forced to move eastwards, southeastwards, southwards, southwestwards, and westwards—always outwards, toward the margin of the continent from the center of evolution and dispersion—by the latter.

I call the Japanese Aino-Caucasian—the Aoshima, Miyato dwarf and Tsukumo tall types, including the modern Ainu,—all together by the name Pan-Ainu. Among the Pan-Ainu, the Aoshima type was the first arrival in Japan. This type is found from the earlier stone age of northeastern Japan almost as a pure race, and from the mediaeval stone age of the same as a race mixed with the next type. Nowadays, it lives in Hokkaido as a race mixed with the next type, and in the Kurile islands and in Saghalin almost as a pure race.

The next to arrive or to arise in Japan was the Miyato dwarf type and possibly also its presumed cousins in western Japan. This type is found from the mediaeval stone age of northeastern Japan as a race mixed with the foregoing type; and its presumed cousins are found from the mediaeval stone age of western Japan as a race mixed with the following type. Nowadays, it lives in Hokkaidô as a race mixed with the foregoing type, and in northeastern Japan and in the north central part of the main island as a mixed race more or less Mongolianized; and its presumed cousins live in southwestern Japan as a mixed race more or less Mongolianized. This type and its presumed cousins are separated from each other in recent distribution by the two following newcomers.

The third to arrive or to arise in Japan was the Tsukumo tall type. It is found from the mediaeval stone age of western Japan as a race mixed with the presumed cousins of the Miyato type. Nowadays, it lives in the northern part of western Japan as a mixed race Mongolianized; and also scattered in every part of Japan, for this type appears to be very common in the former knight class of Japan.

The last newcomer to Japan was the Okayama type of the Mongolian stock. This type is not yet actually found from the

stone age of Japan. Nowadays, it occupies middle western Japan including the former capital of Japan and its vicinity.

The racial and cultural assimilation had been carried on just the reverse of the racial arrivals. It would appear that the Miyato dwarf type had assimilated the Aoshima type to a certain extent in northeastern Japan, while its presumed cousins had been assimilated to a great extent by the Tsukumo tall type in western Japan. There are certain reasons for assuming that the embryonal empire of Japan had been founded by the Okayama type, or by the union of this type and the surrendered aboriginals of western Japan, who consisted of the Tsukumo tall type and the presumed cousins of the Miyato dwarf type. A large part of the surrendered aboriginals served as the warriors of the Imperial army. These warriors invaded eastern and northeastern Japan conquering and assimilating, step by step, the wild aboriginals of these districts. Thus, the type, which has lost its national independence last, was the very Ainu who were the first to arrive in Japan.

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BOOK REVIEWS

METHODS AND PRINCIPLES

Vererbung und Auslese. Grundriss der Gesellschaftsbiologie und der Lehre vom Rassedienst. WILHELM SCHALLMAYER. Dritte, durchwegs umgearbeitete und vermehrte Auflage. Jena: Gustav Fischer, 1918. XVI, 536 pp.

Dr. Schallmayer is known as one of the most energetic champions of *Rassehygiene* or eugenics, and the book now presented in a revised form ranks in Germany as one of the standard works the movement has produced. It is undoubtedly a comprehensive and thorough introduction to eugenic philosophy, especially valuable for the full bibliographic references to relevant literature. On the other hand, the mania for citing authorities and even subjective utterances of eminent writers detracts from the readableness of the book, which further loses through needless detail in the discussion of special points. Thus, the discussion of medical technique in connection with health certificates for bridegroom and bride (398 seq.) seems quite uncalled for.

While Schallmayer is inevitably subjective in framing his ethical aspirations, his views generally commend themselves by an unusual measure of sanity. For a eugenist his position on the race question is remarkably temperate. He does not accept culture as a safe index of racial ability (p. 190) and devotes a whole chapter to an appreciation of the Chinese (pp. 282–310). He specifically repudiates the cult of Gobineau and refuses to identify eugenic aims with exclusive attention to the Nordic elements in the population (pp. 269, 375–387). On other specifically eugenic questions, such as birth control (p. 493), Schallmayer likewise assumes a moderate position.

The author's political ideals have a distinctly liberal bias, especially when contrasted with those of some of his fellow-eugenists in our midst. For example, material success is not accepted without considerable qualification as proof of inherited worth (pp. 146, 226). Schallmayer is willing to go rather far in the direction of democracy and rightly remarks:

> Eine *gesunde* Demokratie schliesst eine Leistungsaristokratie nicht nur nicht aus, sondern ist ohne eine solche überhaupt nicht möglich (p. 462).

A somewhat curious conception of a European league of nations is offered (pp. 494–500). The primary motive is the preservation of Europe from Asiatic and American encroachments. Russia and England are to be excluded because their coöperation would render the formation of the league more difficult and diminish its internal strength.

Ethnologically the author lapses into occasional naïveté. Exogamy is twice defined as a prohibition against marriage within the tribe, *Stamm* (pp. 7, 393). And the pedigrees of the Samoans (p. 388) are not likely to shed much light on problems of heredity.

On the whole, the book may be recommended as a temperate exposition of the eugenic point of view.

ROBERT H. LOWIE

NORTH AMERICA

Seneca Fiction, Legends and Myths: collected by JEREMIAH CURTIN and J. N. B. HEWITT. Edited by J. N. B. HEWITT. (Thirty-second Annual Report of the Bureau of American Ethnology.) Washington, 1918.

This is one of the most important collections of traditional narratives from any native American people both as regards quantity and quality. Curtin's data were collected from 1883 to 1887, mostly in English, Hewitt's in 1896 in Seneca. Two of Hewitt's legends are in text with interlinear translation; the others are given in English only. More than three-fourths of the narratives are Curtin's but nearly half the bulk is from Hewitt. As the average length of the one hundred and thirty-eight stories is about six pages, it is evident that the rendering is full and that nothing has been lost through a desire to hurry through to a gist of the narratives. The native flavor is strong. Curtin's versions, although obviously somewhat less close to the original, hold up excellently in this regard, while Hewitt's must be regarded as models. They remind in many ways of the famous Algonkin translations of William Jones. There is no doubt that a certain quality of English text can be attained only by a recorder who possesses an intimate knowledge of the native language of his informants, such as the majority of field ethnologists in this country are far from possessing. This statement is not to be considered as suggesting that the majority of our Indian traditional material is worthless. For comparative purposes bulk of data and geographical inclusiveness are indispensable. It is far better that we should have collections of tales lacking in literary flavor than not to

have them at all. Yet in the face of a volume which like this one is at once monumental and faithful in its reproduction of the native style, it is well to recognize its unusual virtues.

Hewitt has divided the narratives into fiction, legends, and myths, plus some traditions and tales. In his introduction he discusses the development and relations of these several types. It is doubtful how far this classification would be applicable among other peoples. Some of Hewitt's criteria no doubt do apply elsewhere, and yet the attempt to schematize rigorously would be likely in most cases to lead to artificiality. For ordinary purposes it will continue to be most practicable to assemble all material from one people and divide it so far as may be on the basis of distinctions which they themselves have worked out, or which may be readily apparent in the given case.

Much the same applies to Hewitt's other theoretical point, namely that much of the collecting of native traditions has been so hasty as to be unfavorable to the acquisition of the more philosophic and poetic creations, and that it has frequently been accompanied by an overaccentuation of the coarse and obscene. The first part of this criticism can be met with considerations similar to those just discussed in regard to style. It is certainly desirable that we record the finest specimens of the product of the native mind. The search for values in civilization has the greatest importance, yet has often been ignored or looked upon askance as something unscientific. On the other hand there are results other than values or qualities that can be derived from cultures. The interest in the actualities of a civilization is as justified as in its idealities, and for purposes of tracing historic development and determining causes, it is indispensable that the material available be both as full as possible and free from selection by any standard of quality. It will be to the advantage of both lines of work if students devoted primarily to each will meet the efforts of the other camp with full sympathy.

As regards the point of coarseness in native tradition, which Hewitt revives, we seem to have come to the stage where one group charges the other with being obscene-minded and this party retaliates with the accusation of unscientific prudishness. Here again a recognition of the value of each method of approach appears called for. It seems worth while only to add that there undoubtedly exist differences in temperament of nations as well as observers. If Hewitt's work had lain wholly among Pacific Coast instead of Iroquoian tribes his attitude would probably have been less positive. Part of the criticism which he directs against observers attaches to the tribes with which fortune has

thrown them into contact. Nevertheless Hewitt's discussion is stimulating and well worth while as a reminder that two attitudes are entertainable.

All in all, this is a notable piece of work and arouses the lively hope that it may continue to be followed, and soon, by others from the pen of the part author and editor of the whole.

A. L. KROEBER

OCEANIA

The Hawaiian Romance of Laieikawai, with Introduction and Translation. MARTHA WARREN BECKWITH. (Thirty-third Annual Report of the Bureau of American Ethnology, pp. 285–666.) Washington, 1919.

The Laieikawai is a Hawaiian romance, heavily flavored with mythology, epic in length and treatment, but with the love interest dominant. It contains many songs, but is mainly in prose. How long it had been preserved orally is not known. Haleole, a native, published it in Hawaiian in 1863 that there might "abide in the Hawaiian people the love of their ancestors and their country." The theme as well as the language were adapted by Haleole to his day; yet the modifications introduced by him into the ancient tale appear to be very slight. It is the longest and in many ways the greatest piece of Polynesian literature preserved. The plot, seemingly inchoate at first, develops through six hours of recital or 137 pages of print with ever-increasing inner unity and magnificence of conception. The tale is a monument of the civilization that produced it.

The Laieikawai was reprinted in Hawaiian in 1888, but has been available in translation only in greatly condensed versions, in king Kalakaua's book of legends and in an article by Rae in the Journal of American Folk-Lore for 1900. Miss Beckwith gives the full text, an apparently accurate translation, notes on the text, and an appendix of abstracts of other Hawaiian tales collected by Fornander. In her Introduction she reviews especially the mythology and the art of composition involved. The latter section contains much of interest to students of comparative literature. Miss Beckwith's work throughout is done in a scholarly manner; and the intrinsic value of the material which she has made generally available is so great that her painstaking and successful labor deserves grateful recognition.

A. L. KROEBER

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Skinner, Alanson. Medicine Ceremony of the Menomini, Iowa, and Wahpeton Dakota, with Notes on the Ceremony among the Ponca, Bungi Ojibwa, and Potawatomi. (Indian Notes and Monographs, vol. IV.) New York: Museum of the American Indian, Heye Foundation, 1920. 357 pp., 26 pls., 14 figs.

Vormann, P. Fritz. Das tägliche Leben der Papua, unter besonderer Berücksichtigung des Valman-Stammes auf Deutsch-Neuguinea. (Anthropos, vols. XII–XIII, 1917–1918, pp. 891–909.)

Waterman, T. T. *See* Kroeber, A. L.

Weule, Karl. Zusammenhänge und Konvergenz. Ein Wort zu F. v. Luschans Glaubensbekenntnis. (Petermanns Mitteilungen, 1920, pp. 69–77, 1, pl.)

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Willoughby, Charles C. *See* Hooton, Ernest A.

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DISCUSSION AND CORRESPONDENCE

Africa and the Discovery of America

I wish most emphatically to protest against Professor Dixon's unique method in attacking my book, *Africa and the Discovery of America*. I do not refer to his inability to accept my views, but to the manner in which he represents them to readers of the *Anthropologist*. I decline to answer in the same abusive language as that used by him, and it is impossible for anyone to answer generalities. Fortunately Professor Dixon has made a number of specific charges, and all of these I shall analyze here.

Prof. Dixon says (p. 179):

> In one at least of his attempts to prove the "atrocious forgery" of much of Columbus' writings and those of Ramon Pane, Professor Wiener shows a readiness to seek for and accept far-fetched explanations, a tendency which becomes more noticeable in his later chapters. Thus he rejects as a lie the story told by Columbus of fishing by the aid of the remora or sucking-fish, and declares it to have been derived from Odoric of Pordenone's account of cormorant fishing in eastern China. A little investigation would have shown that improbable as it may seem, there is no good reason to brand it as a pure invention or plagiarism, for precisely this same method has been and still is employed in Melanesia and its practicability has recently been demonstrated by tests in New York.

I am not dealing with the probability or improbability of the remora fishing, but with the *literary lie* connected with it. On p. 55 I say:

> Our purpose, however, is not to get at the correct text of Columbus, but to study the manner in which the errors were perpetuated and finally received the sanction of Columbus himself, who thus became a coadjutor in the forgeries.

On p. 61 I refer to the fishing story as follows:

> We can study the formation of another ghost word in a passage contained in Peter Martyr and Bernaldez, and, although this ghost word is not necessarily due to Columbus, the lie told in connection with it was published in 1504 in the *Libretto*, and Columbus never took the trouble to deny it.

I three times specifically absolved Columbus of this lie (pp. 61, 65, 67). I showed that Peter Martyr's *guaicanum*, the native name of the fish caught by the remora, was a ghost word, arising out of Bernaldez's *caza*, and was trying to get at the basis of "the older original from which Bernaldez got his version." I showed how the well-known cormorant

story in Odoric was in the Italian version changed into that of the remora, which puzzled Yule before me, for he said, "This (Italian) edition has in this passage an exceedingly curious variation, difficult to account for."[1] One can only be puzzled at the attempt of the gentlemen in New York to prove the veracity of the remora story, since it has been very frequently recorded. Mas'ūdī, Ibn Batūtah, Idrīsī, Al-Qazwīnī, and Ad-Damīrī hint at the catching of the wāl off the shore of Zanzibar by means of the remora, and the works of Santos, Dampier, Commerson, Salt, and Middleton tell specifically of the turtle fishing by means of the remora on the east coast of Africa, from Zanzibar to Natal. In fact, that the whole story in Bernaldez and in the writings based on Columbus' *Second Voyage* is a base lie, is proved, beyond any possibility of cavil, from an entry in the Swahili Dictionary:[2]

Kassa, turtle, of which there are various kinds. . . . The *kassa* is caught by means of the táza fish, which the fishermen carry alive with them. When they see a *kassa*, they let the taza go after it, to stick fast to the *kassa*. When the taza has seized it, the fisherman throws a harpoon and takes the *kassa* out of the sea, the taza letting go instantly when exposed to the air.

This shows that Bernaldez's *caza* is taken out of an account about Zanzibar, and either there was an Arabico-African influence in America before Columbus, or the whole story is a huge lie.

Again Prof. Dixon says (p. 179):

As the earliest certain record the author has been able to discover referring to the use of tobacco in Africa is at the end of the sixteenth century, it is obviously incumbent on him, if he is to prove his theory, to find indirect evidence of its earlier presence.

In the index of my book may be found the entry: "*Mentioned in Africa in the fifteenth century*, p. 111," and on that page I say:

An Arabic source speaks of it as in use in the middle of the ninth century of the hegira, which would be about the middle of the fifteenth century; this date, however, is not certain and needs verification.

I give the reference in the footnotes: Cap. Binger, *Du Niger au Golfe de Guinée*, Paris, 1892, vol. II, p. 364. Here we read:

M. Houdas a encore eu l'amabilité de nous donner la note cijointe, qui confirmerait bien que le tabac est, sinon originaire du Soudan, au moins qu'il y est connu depuis les temps reculés. 'A Koubacça, le tasert aussi de monnaie. Par

[1] Sir Henry Yule, *Cathay and the Way Thither*, London, 1913, vol. II, p. 189.
[2] L. Krapf, *A Dictionary of the Suahili Language*, London, 1882, p. 130 f.

une singulière homophonie avec le nom européen, les habitants du Dârfour l'appellent, dans leur langage, *taba*. Bien plus, ce nom de *taba* est commun dans tout le Soudan. Au Fezzan et à Tripoli de Barbarie, on l'appelle *tabgha*. J'ai lu une cassidah, ou pièce de vers, composée par un Bakride ou descendant de la famille du khalife Abou Bakr, afin de prouver que fumer n'est pas pêcher. Ces vers, je crois, datent d'environ le milieu du IX^{e} siècle de l'hégire. En voici quelquesuns: Dieu tout-puissant a fait sortir du sol de notre pays une plante dont le vrai nom est *tabgha*. Si quelqu'un, dans son ignorance, te soutient que cette plante est défendue, dis-lui: Comment prouves-tu ce que tu avances? Par quel verset du Coran?

Since Houdas was not absolutely sure of the date, which might as easily be earlier than the middle of the fifteenth century as later, I advised caution as regards the date. That there are no other sources for smoking in Africa in the fifteenth century is only natural: there are no works whatsoever of the fifteenth century that deal with the manners of the Africans.

In a footnote on page 180 Prof. Dixon says:

> Of the two references given to prove that tobacco is native in Africa, one does not even refer to the subject, while the other clearly indicates the exact opposite to what Prof. Wiener says. Similar examples of gross carelessness or direct misrepresentation abound.

This refers to my statement (p. 111): "The *Nicotiana tabacum* grows wild in Africa, and so does the *Nicotiana rustica*." For the first the footnote reads: "F. Welwitsch, *Catalogue of the African Plants*, London 1898, vol. I^{2}, p. 754." In Welwitsch's work, where plants are distinguished as "cultivated," "escaped from cultivation," and "wild," the entry is: "*N. Tabacum*. Icolo e Bengo. At the banks of the river Bengo, near Funda, *wild*." The second has the footnote: "G. Schweinfurth, *Im Herzen von Afrika*, Leipzig, London, 1874, vol. I., p. 295." The page reference, by oversight, is given as 295, instead of 279, but this does not excuse the reviewer's statement, since there are only two references to "tobacco" in the Index, one of them being the following in the English translation: [1]

> Quite an open question I think it is, whether the *N. rustica* is of American origin. . . . Barth has given his opinion that the tobacco is a native of Logane (Mosgoo). . . . The conjecture is tenable, that they probably favoured the propagation of the foreign growth, because smoking, either of the common tobacco (*N. rustica*) or of some other aromatic weed, had in some way already been a practice amongst them.

[1] E. E. Frewer, *The Heart of Africa*, London, 1873, vol. I, p. 255.

Barth[1] says:

We had already seen much cultivation of tobacco in this country, and were impressed with the opinion, however strange it may seem, that it was an indigenous plant, and not introduced at a recent period.

Prof. Dixon continues (p. 180):

The second stage in the argument, *viz.*, that we have no early accounts of tobacco or the use of smoking in America is equally unconvincing. He points out what is indeed a puzzling fact, that Columbus in his first voyage makes but one very uncertain reference to smoking, and that in the earlier accounts of Florida its use is not mentioned. On the other hand he minimizes and quite misunderstands (as well as mistranslates!) the evidence afforded by Sahagun and Bernal Diaz.

In regard to Bernal Diaz I said (p. 126): "Bernal Diaz del Castillo says that after Montezuma had partaken of his dinner he smoked liquid amber wrapped in leaves called *tabaco*, which put him to sleep," and quoted the Spanish passage: "También le ponían en la mesa tres cañutos muy pintados y dorados, y dentro traían liquidámbar revuelto con unas yerbas que se dice *tabaco* y cuando acababa de comer, después que le habían cantado y bailado, y alzada la mesa, tomaba el humo de uno de aquellos cañutos, y muy poco, y con ello se dormía." Let any Spanish scholar show that I "minimize and quite misunderstand (as well as mistranslate!)" the passage. There is no reference in Bernal Diaz to smoking, but only to sniffing incense. This is in complete agreement with Grijalva's account, in 1518, as recorded by Oviedo:[2]

And the chief Indian . . . gave to each of the Christians, who were seated, a small tube which was burning at one end, which are made in such a way that after they are lighted they slowly diminish and are consumed until they stop burning, without giving out a flame, just as the incense sticks of Valencia do, and the smoke which came out of them gave forth a fine odor. And the Indians made signs to the Christians that they should not allow any of the smoke to pass away, *as when one takes tobacco.*[3]

[1] *Travels and Discoveries in North and Central Africa*, London, 1857, vol. III, p. 229.

[2] *Historia general y natural de las Indias*, Madrid, 1851, vol. I, p. 525.

[3] "Y el general, con los que el indio prinçipal señaló, sentados, dió este al general é á cada uno de los chripstianos que estaban sentados un cañuto encendido por el un cabo, que son fechos de manera que despues de encendidos poco á poco se van gastando é consumiendo entre sí hasta se acabar ardiendo sin alçar llama, assi como lo suelen haçer los pivetes de Valençia, é olian muy bien ellos y el humo que dellos salia: é haçian señas los indios á los chripstianos que no dexassen perder ó passar aquel humo, como quien toma tabaco."

It is clear from the last sentence that the Indians did not smoke tobacco, but were only sniffing incense. That this account is correct is proved by hundreds of representations, in the Aztec Manuscripts, of the priest with the *yetecomatl*, held in his hands, the finest being the picture of Montezuma; there being no case on record in which the tube is held in the mouth for smoking.[1] The quotation from Sahagun is too long for reproduction. It is given by me on p. 148 f. The reader may compare it with the original and convince himself that my translation is substantially correct. To this I shall return further on.

Again Prof. Dixon says (p. 180):

He ridicules Oviedo's earlier errors in confusing the Antillean custom of inhaling cohoba (*Piptadenia peregrina*) with the smoking of tobacco, and denies *in toto* the former practice with its use of the bifurcated snuffing tube; a denial which, in view of Uhle's and Safford's careful studies, is without force.

In my book I say (p. 113):

Neither Ramon Pane nor Columbus refers to the *smoking of cogioba*, but only to its use as an inhaled powder.

Is this a denial *in toto*, etc.? As to the bifurcated reed, I say (p. 107):

From all this it follows that the implement used for errhines was a small funnel, which would be represented as Y, and which Columbus, who apparently saw the illustration in a book, mistook for a forked reed. It was the thinner end which was inserted in the nose, and it is not practicable to devise a forked reed, in order to insert the fork into the nose. Certainly no such implement could have been used for smoking, as has been shown by experiment.

At the time of writing I did not know of Uhle and Safford. I am again grateful for being reminded of authorities which confirm my deductions in every detail. Safford[2] shows conclusively that neither Columbus nor Ramon Pane described *smoking of tobacco*, but snuffing of a narcotic, which is precisely my argument. Safford, too, ridicules Oviedo, and ends with the words:[3]

Oviedo, unfortunately, has been quoted by many authors, and his Y-shaped figure, with its branches so diverging that they could not possibly have been simultaneously inserted in the nostrils of a human being, has been copied again and again.

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[1] This matter I shall treat more fully in my second volume.

[2] Identity of cohoba, the narcotic snuff of ancient Haiti, in *Journal of the Washington Academy of Sciencies*, vol. VI, p. 547 ff.

[3] *Ibid.*, p. 549.

The next passage in Prof. Dixon's review is too long to reproduce here. It ridicules my philological derivation of the Tupi-Guarani words for "tobacco" from Lat. *bitumen*. I ask the reader to read the passage and compare it with my deduction (p. 135):

At the end of the fifteenth century the Arabic influence in medicine was not yet extinct in Portugal and Spain, and if the Arab. *tubbāq* passed into their languages in all its different significances connected with that aromatic plant, then *betume* must have acquired the same various meanings. It is this word which was taken by the Portuguese, with the tobacco plant, to Brazil, at the same time that Portuguese *matraca* entered into Tupi.

What about *matraca?* Is this, too, ridiculous? Prof. Dixon says (p. 181):

It was this word which, with the plant itself, had been taken by the Portuguese pilot of Pigafetta to Brazil more than thirty-five years before! Comment seems superfluous.

What I say is this:

The latter fact (the entrance of *matraca* into Tupi) happened before 1519, possibly through that very pilot who accompanied Pigafetta on his Voyage around the World.

In the next onslaught, too long to quote here, my veracity in quoting from Jaques Cartier is assailed, with the following comments. "'*Prunes*' in English is not the equivalent of '*prunes*' in French!" This is news indeed. Any dictionary will show that Fr. *prunes* means both plums and prunes. If necessary, the word *sèches* is added to distinguish the second from the first. "The use of 'apples' in Cartier's text is, as Professor Wiener failed to note, due to a misprint of 'pommes' for 'prunes'." *Pommes* occurs twice after *plums*:[1] "They likewise have *plums*, which they dry as we do for the winter, which they name Honesta; figs, nuts, pears, *apples*, and other fruits, and beans, which they call Sahe; nuts, Daheya; figs, Honnesta; *apples*." Consequently this cannot be a misprint for *prunes*. Besides, the facsimile MS. of Jaques Cartier's book has distinctly *pommes*, and not *prunes*.[2] Prof. Dixon says (p. 181):

Cartier does *not* say that the Indians had names for all of the seven articles which he enumerates, and he gives the names only for four of them, viz., figs., plums ("prunes" in English is not the equivalent of "prunes" in French!) cloves and cinnamon.

[1] J. P. Baxter, *A Memoir of Jacques Cartier*, New York, 1906, p. 110 f.
[2] *Ibid.*, pp. 289, 290.

I say (p. 137):

> At the same time he mentions figs, cloves, and cinnamon, oranges, almonds, and apples, as known to the Indians and possessing Indian names.

I did not say that six (not seven) articles all had Indian names; but they were all known to the Indians, and, of course, if the Indians understood Cartier, they all must have had Indian names. But I continued: "There is one circumstance which casts a doubt on the whole story." So I did not vouch for anything. Prof. Dixon's assumed identification of plums, cinnamon, etc., with American plants is taken bodily out of Baxter,[1] but both are thoroughly mistaken. Prunes, figs, almonds, nuts, etc., were carried by all the voyagers in their ships, and Indians constantly received these fruits from the Europeans. Indeed, we hear of these from the very earliest times after the discovery. In 1494 ships carried almonds and olives to America,[2] and it will be observed that all MSS. of Cartier give an Indian name for "olives," namely *houocohonda*.[3] In 1500 Alvares Cabral gave the Brazilian Indians some *dried figs*.[4] *Prunes* are constantly mentioned in the *Jesuit Relations* as an Indian food, and more than once the specific statement is made that French prunes are meant: "Besides, they get from our French People galette, or sea biscuit, bread, prunes, peas, roots, figs, and the like. You have here the food of these poor people" (1634).[5] This proves conclusively that Cartier had chiefly in mind imported articles of food. Prof. Dixon identifies "almonds" with "several varieties of nuts"; but in Cartier "almonds" is immediately followed by "nuts," which makes the identification impossible.

Prof. Dixon says (p. 182):

> There remain the "cloves" and "cinnamon." In regard to the former, it is to be noted that they are referred to in two of the three manuscripts as "so-called cloves," obviously indicating that they resembled but were not true cloves.

The sentence before the "cloves" reads: "Note that their lord named Donnacona has been to a land where they are, a moon going with their boats from Canada to the said land, in which there grows much cinnamon and cloves."[6] This is followed in two manuscripts by "ladicte

[1] *Op. cit.*, p. 111.

[2] M. F. de Navarrete, *Coleccion de los viages y descubrimientos*, Madrid, 1825, vol. II, p. 151.

[3] Baxter, *op. cit.*, p. 212.

[4] *Alguns documentos do archivo naciona' da Torre do Tombo*, Lisbon, 1892, p. 110.

[5] Vol. VI, p. 273.

[6] Baxter, *op. cit.*, p. 214 f.

canelle," which Baxter blunderingly translates by "they call *the said cloves* adhotathny," while Prof. Dixon perpetuates Baxter's blunder, by translating *canelle* as "cloves," and in addition gives to Fr. *ladicte* a meaning which it does not possess, in order to make a point against me.

Prof. Dixon says (p. 182):

The theory that the Iroquoian tribes north of the St. Lawrence were, in the early sixteenth century, in direct trade relations with the Gulf of Mexico could, quite apart from its inherent improbability on account of the distances involved, only be imagined by one quite unaware of the character of Indian trade and of the political conditions among the eastern tribes at this time.

Yet Baxter, whom Prof. Dixon quotes *in toto*, in order to prove Prof. Dixon's assumed identification of plants, has the very opposite opinion. Cartier says (p. 189 f.):

Moreover, they have given us to understand that at the place where we had left our boats when we went to Hochelaga there is a stream that goes toward the southwest, where, likewise, it takes a moon to go from St. Croix with boats as far as to a land where there is never ice nor snow; but that in this said land there are continual wars one with another, and that in this land there are oranges, almonds, nuts, plums, and other sorts of fruits, and in great abundance. And it was told us that the men and residents of the land were clad and arrayed with skins as themselves. After having asked them if there was any gold and copper there, they answered us no. I esteem the said place to be, by their saying, toward Florida by what they showed us by their signs and tokens.

To this Baxter aptly adds the note:

In spite of continual warfare among the different savage tribes, there were many ways by which they could obtain a knowledge of the inhabitants and products of distant regions. Cartier was evidently right in his conjecture that the country described was "toward Florida." It is quite possible that the natives of Canada had intercourse at times, either directly or indirectly, by the great waterways toward the southwest, with the tribes in that direction.

Prof. Dixon can get his information only from the sources from which I get mine, namely from the writers of the sixteenth century, and they unanimously contradict him. The trader and medicine man was exempt from the feud, and traveled unmolested over enormous distances. Cabeça de Vaca wrote:

I put myself to contriving how I might get over to the other Indians, among whom matters turned somewhat more favorably for me. I set to trafficing, and strove to make my employment profitable in the ways I could best contrive, and by that means I got food and good treatment. The Indians would beg me to go from one quarter to another for things of which they have need; for in consequence of

incessant hostilities, they cannot traverse the country, nor make many exchanges. With my merchandise and trade I went into the interior as far as I pleased, and traveled along the coast forty or fifty leagues. The principal wares were cones and other pieces of sea-snail, conches used for cutting, and fruit like a bean of the highest value among them, which they use as a medicine and employ in their dances and festivities. Among other matters were sea-beads. Such were what I carried into the interior; and in barter I got and brought back skins, ochre with which they rub and color the face, hard canes of which to make arrows, sinews, cement and flint for the heads, and tassels of the hair of deer that by dyeing they make red. This occupation suited me well; for the travel allowed me liberty to go where I wished, I was not obliged to work, and was not a slave. Wherever I went I received fair treatment, and the Indians gave me to eat out of regard to my commodities. My leading object, while journeying in this business, was to find out the way by which I should go forward, and I became well known. The inhabitants were pleased when they saw me, and I had brought them what they wanted; and those who did not know me sought and desired the acquaintance, for my reputation.[1]

And how is it improbable that the Iroquoians were in direct trade relations with the Gulf of Mexico, when Cartier distinctly says so, and Sagard a hundred years later repeats the assertion? As to the distances involved, we have a very detailed itinerary for the Indians in the *Jesuit Relations*,[2] from which we learn that they went in their canoes on long voyages, 600 miles at a time, and averaged as high as 60 miles a day and more, going down stream. In a moon's time, going down the Mississippi and its tributaries, the Indians could make 1800 miles, or considerably more than the distance from the Huron country to the Gulf. In a moon and a half, as given by Sagard, they could have gone to any place, as far as the present state of Florida.

Says Dixon (p. 182):

> On page 145 ff. it is contended that the carriers of this trade in tobacco and tropical fruits were the Algonkian people called by Sagard (and by him alone) the Epicerinys, whose name is derived by Professor Wiener with all apparent seriousness from the French 'épicerie' (spices). It is hardly necessary to point out that these "bringers of spices" are the Nipissirini or Nipissings of the lake of that name in northern Ontario.

What I said is this: "It will be observed that the people who told Sagard about this are called Epicerinys, *apparently* from the French *épicerie* 'spices,' that is, 'the people who bring the spices.'" There can be little doubt that Sagard connected the Nipissings with traders

[1] Buckingham Smith, *Relation of Alvar Nuñez Cabeça de Vaca*, New York, 1871, p. 85 f.

[2] Vol. XLIV, p. 237 ff.

of the Sea of Spices, hence he changed *Nipisserini* to *Bisserins*, *Epicerinys*. It must be kept in mind that a large number of Indian names were by popular etymology transformed into French words. Just as the *Hurons* were made to appear as the French *hurons* "tousle-heads," although derived from the Iroquoian *hronon* "man," a word with which many names of nations end in Iroquoian, so *Epiceriny*, "apparently from *épicerie*," is in reality based on some other word. But Prof. Dixon's derivation of *Nipissings* from "Lake Nipissing" is as correct as would be the derivation of *Huron* from "Lake Huron." The Hurons called the Nipissings *Skekwanenhronon* "Sorcerers," and the French, too, called them "Sorcerers." This makes it certain that their Algonkian name was *Nipikiwinen* "sorcerers," from which developed *Nipissirinen*, etc. But what in the world did the Nipissings have to do with my statement? I showed that for one hundred years the Hurons, or some people among them, traded with a people on the Gulf. Cartier told this of Donnacona, Sagard of the Epicerinys. Without committing myself to their identification, I merely showed that the traders were "the spicerers" of the north, and "spicerer" in the Middle Ages meant "dealer in imported fruits, etc."[1] The matter is not in the least changed, if the popular etymology is wrongly taken; hence I added the word "apparently."

To continue with Prof. Dixon's criticism (p. 183):

> Space is lacking to point out all the vagaries which fill the pages of this extraordinary chapter. These range from misstatements, such as when (p. 189) it is said that Alarcon in 1540 described Indians of the Northwest (sic) as "addicted to smoking, carrying the tobacco and the pipe in a bag tied to their arms, to the quite incomprehensible attempt to make the Mexican 'chapopotli' (which was by the author's own statements, a bituminous, reddish-purple, aromatic material mixed with other substances in the filling of cigarettes) equivalent to meerschaum."

From here on Prof. Dixon's crabbed pronunciamentos make it hard to ascertain what his censure is. I shall try, however, to answer the vague charges as far as I can make them out. As to the Northwest, a little imagination will show that my geographical terms are those of the period to which I refer. When I speak of Florida, I mean the territory north of the Gulf of Mexico up to the fortieth degree of latitude. Similarly, the Pacific region north of Mexico is the Northwest of the sixteenth century. I had to choose some such expression, in order to include both Alarcon, who went up the Colorado River, and Drake,

[1] See my Economic History and Philology, in *Quarterly Journal of Economics*, vol. XXV (1910), p. 275 ff.

who went up to latitude 38°, as well as Fletcher, who speaks of latitude 48°. I specifically pointed this out in the concluding sentence, "that *tabaco*, first mentioned in Hispaniola, should have found its way so far to the northwest, in addition to the rest of the continent, is a *prima facie* proof that the distribution of *tobacco* follows from its first appearance under Arabic influence, from Guinea to all countries where Spanish, Portuguese, and French sailors navigated via Guinea or after having taken part in Guinea expeditions" (p. 141). Prof. Dixon makes me say that Alarcon spoke of tobacco. On p. 141 I said, "It is not certain that Alarcon here described the tobacco." On p. 189 I made the concession to Prof. Dixon's school that it was tobacco, even as it is mentioned in the margin in Hakluyt. Although I counseled caution, I am now inclined to believe that that was exactly what Alarcon meant, because the early expression for "smoking" was "to drink tobacco," as it still is in Arabic and other languages. Besides, we have an almost contemporary reference in Spanish to "drinking of tobacco." Herrera gives, under the date of 1550: "Tambien usan mucho del Tabaco, para Reumas, Corrimientos, i dolores de Cabeça, i lo toman molido en polvo, por las Narices, i beben el çumo, i los hace purgar, i tambien lo usan los Castellanos." Tobacco was taken as snuff, through the nose, and the juice was drunk, as the Spaniards do, that is, it was smoked. If it was drunk, it was still tobacco that was drunk.

The modern meerschaum dates from the eighteenth century, and the misnomer "sea-foam," since it is mined and not found in the sea, has been a puzzle to philologists. Sahagun and Belon, however, help us over the difficulty. The modern meerschaum is merely a substitution of the real *meer-schaum*, the old German name of the *alcyonium durum*, a pitchy, pumice-like substance of the sea, which was used in the manufacture of aromatic tobacco pipes (*cf.* my book, p. 148 f.). The confusion with the pissasphaltum of the ancients was due to Sahagun's following explicitly Belon's explanation. The modern meerschaum obviously came into use because of its property to discolor beautifully, exactly as Sahagun's chapopotli and Belon's pissasphaltum did, and this is the only justification for its absurd name. Had I been writing a disquisition on *meerschaum* and not on tobacco, I should have made the matter clearer, but all I wanted was to show the important fact that Sahagun got his description, not from an Aztec source, but from Belon.

My critic speaks of

> the credulity which accepts without question Squier's identification of "manatee" and "toucan" pipes in the Ohio mounds (p. 168) to the assurance which, in utter

disregard of all archaeological data, declares the pottery heads of San Juan Teotihuacan "negroid" and hence post-Columbian.

No doubt, Prof. Dixon has in mind H. W. Henshaw's censures of Squier and Davis in his *Animal Carvings from Mounds of the Mississippi Valley*,[1] but even he did not speak so disparagingly of Squier. Besides, Henshaw's conclusion,

> that a large majority of the carvings, instead of being, as assumed, exact likenesses from nature, possess in reality only the most general resemblance to the birds and animals of the region which they were doubtless intended to represent,[2]

leaves the subject wide open, and, in spite of occasional errors in Squier and Davis it is much safer to accept their verdict, than Henshaw's. As to the post-Columbian origin of the negroid pottery head, there is not a word about it in my book. I only quoted Charnay, who said that he picked up a Negro head, and that the *ruins were still in use in Spanish times*.

The other censures by Prof. Dixon are too vague to be aswered here. They will be properly illustrated in my future works.

LEO WIENER

A REJOINDER

THE Editor has kindly given me the opportunity to add a few words in the way of rejoinder to Professor Wiener's lengthy criticism of my recent review of his book. Ordinarily I should be quite content to leave the verdict as to the value of the book, the validity of my criticisms, and the adequacy of Professor Wiener's reply to any anthropologists who cared to waste their time over the task. But (and I regret to be obliged to speak thus plainly) the disingenuousness of Professor Wiener's criticisms and the fact that he has stooped to the employment both of *suggestio falsi* and *suppressio veri* practically force me to make a brief answer. I shall do this by the shortest possible comments on the more important points at issue.

1. Remora fishing:—On pages 61–67 of his book, Professor Wiener ridicules the whole idea of remora fishing, and attempts to prove the practice unknown in the West Indies and the accounts of it "lies." In his reply, he again obligingly refers to these accounts as "base lies." He refers to no purpose to eighteenth century descriptions of the practice on the East African Coast, but carefully omits any references to ac-

[1] In *Second Annual Report of the Bureau of American Ethnology*, 1880–81, p. 123 ff.
[2] *Ibid.*, p. 166.

counts of the use of the remora in Caribbean waters. The whole question has been exhaustively treated by Gudger (*American Naturalist*, LIII, pp. 289, 446, 515) to whose paper those interested may be sent.

2. Earliest use of tobacco in Africa:—My statement was that the earliest *certain* record given was toward the end of the sixteenth century. In his reply, Professor Wiener has only succeeded in making it more deadly clear that my statement was quite correct!

3. Tobacco native in Africa:—Welwitsch does indeed, as Professor Wiener states, record *Nicotiana tabacum* as "wild" at one station, but Professor Wiener does *not* state two facts of vital importance in estimating the value of Welwitsch's evidence for this species of tobacco being *native* in Africa. These are: (1) that the station referred to is but a short distance from Saõ Paulo de Loanda, the capital of Angola, a town which has been in existence since the latter part of the sixteenth century, and (2) that Welwitsch also gives a *second* station in the same area, with the entry "cultivated and afterwards sporadically and rather rarely half-wild." The mere fact that a plant is found growing "wild" is not, as any botanist knows, evidence that it is *native* in the region. As tobacco is a plant that somewhat easily escapes from cultivation and naturalizes itself, no competent botanist would, I think, accept Welwitsch's *two* statements, as in any sense proof that *Nicotiana tabacum* was *native* in an area where it has been cultivated for several centuries.

As to Schweinfurth, Professor Wiener is obliged to admit that his reference made no mention of the subject. I was quite well aware of the statements elsewhere in the book, but I did not and do not regard "open questions" and "tenable conjectures" as constituting valid evidence, particularly when these "conjectures" go counter to the currently received scientific opinion.

4. Bernal Diaz and Sahagun:—I still believe Professor Wiener "minimizes" the evidence of Bernal Diaz which he gives—and he does not give it *all!* The interpretation of Grijalva's account (which seems previously to have escaped Professor Wiener's attention) does not seem to me to be defensible. Professor Wiener wisely omits to quote the passage from Sahagun which I said he had misunderstood and mistranslated. The French text, speaking of the making of reed cigarettes (not "pipes" as Professor Wiener translates it) reads "il pulverise ensuite très finement le charbon qu'il mouille et dont il bourre les roseaux." This is rendered (p. 148 of Professor Wiener's book), "Then he carefully pulverizes some *coal*, which he dampens *in order to bore with it through* the reeds" (italics mine). Let any French scholar show that this is

not a hopeless misunderstanding and egregious mistranslation of the original. The reeds were filled or stuffed with dampened charcoal, not "bored" with "coal."

5. Inhaling cohoba:—An excellent example of disingenuous method. Professor Wiener artfully quotes a sentence of his from page 113 of his book, *without its context* which completely modifies its meaning, and then asks in injured innocence if "this is a denial *in toto*." Of course it is not—but the whole of the nine pages which precede are given up to nothing but an elaborate denial of the use of cohoba and the bifurcated snuffing-tube, and an attempt to prove both Columbus' and Ramon Pane's accounts of these to be pure lies! He then quotes Safford, cleverly but quite falsely suggesting that this quotation sums up Safford's views on the *whole question*—but very carefully omits any reference to the succeeding thirteen pages, in which Safford demonstrates the wide extension of both practices and their use in prehistoric times.

6. Bitumen:—Comment, even on Professor Wiener's comment, again "seems superfluous."

7. Cartier:—Fr. *prune* = plum; Fr. *pruneau* = prune. This is an old catch for beginners in French. If Professor Wiener can refer me to "any dictionary" which gives both meanings as in good use for Fr. *prune*, I shall be very grateful. In speaking of the misprint of *pommes* for *prunes* I was referring to the specific case of its appearance in the vocabulary. That the word for apple (a native fruit) occurs *elsewhere* in Cartier's text has no bearing whatever on the question. I cannot take the space necessary for an adequate discussion of the other points raised.

8. Epicerinys:—I would only point out that I never dreamed of deriving "*Nipissing*" from "Lake Nipissing." I merely stated the fact that the tribe called *Nipissirini* in the seventeenth century are today called *Nipissings* and that they lived near a lake which bears their name. I am quite content to leave Professor Wiener's derivation from the Fr. *épicerie* to the tender mercies of other philologists!

9. Alarcon:—The "misstatement" lies in the fact that Alarcon does not mention either tobacco or pipes. Although fifty pages or so earlier, Professor Wiener admits that it is a pure assumption that tobacco was meant and could adduce no evidence whatever in regard to pipes, yet on the page referred to (p. 189) he states both assumptions as *facts* without qualification, in order to bolster up his theory.

10. Chapopotli:—There is not space here to discuss the complete muddle into which Professor Wiener has fallen in this matter. Anyone who cares to read pp. 148–149, 181–184 of his book, can convince himself of this.

11. Ohio Effigy-pipes:—Professor Wiener here again employs both *suppressio veri* and *suggestio falsi* with a master hand. The question at issue is whether or not certain pipes found in Ohio represent the manatee and the toucan, animals and birds whose nearest habitat was in the Gulf of Mexico and the regions south. In order to give the impression that Henshaw's criticisms do not invalidate but rather confirm the accuracy of Squier and Davis' identification, Professor Wiener carefully quotes Henshaw's *second* conclusion, and then states that the latter thus "leaves the subject wide open." He deliberately *omits*, however, to give Henshaw's *first* conclusion which immediately precedes and which reads "that of the carvings from the mounds which can be identified there are no representatives of birds or animals not indigenous to the Mississippi Valley." In the body of his article, Henshaw has just shown conclusively that Squier and Davis' identifications of "manatee" and "toucan" pipes are absolutely untenable!

12. Teotihuacan "negroid" heads:—The *word* "post-Columbian" is to be sure not used in connection with these heads—but the entire paragraph in which they are mentioned, has for its primary purpose to suggest that the heads are "negroid" because they were made subsequent to the introduction of Negro slaves.

Professor Wiener's reply constitutes neither fair criticism nor legitimate argument; it is rather an evasion of the issues and a specious presentation of irrelevant or misleading facts.

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THE REINDEER

MY "Notes on Reindeer Nomadism" (*Memoirs of the American Anthropological Association*, Vol. VI, 2) has called forth a reply from Dr. Laufer in the Anthropologist's April-June number, 1920, which has just come to hand.

I am heartily in accord with Dr. Laufer when he says "that facts mean everything." Facts should, however, be rightly understood; therefore, we must attempt to find out the right order in which the facts should be arranged so as to be fully comprehended. That is where theory comes in. Now, Eduard Hahn and after him Dr. Laufer have not, in my opinion, arranged the facts in the right order; therefore, I have tried to arrange them better. From Dr. Laufer's reply I learn of his disapproval of my analysis of reindeer nomadism and my attempt at a chronological stratification of the elements contained in this culture-

form. "The criteria made out for earlier and later phenomena are purely subjective and a matter of debatable opinion." Unfortunately, Dr. Laufer does not say why he regards my criteria as purely subjective. It would have been interesting to know why Dr. Laufer regards the geographical distribution of the elements of reindeer nomadism as purely subjective. The fact that he finds my work deserving of condemnation is not sufficient basis for a discussion.

When I find reason to suppose that Ørjan Olsen may have made a mistake regarding the taming of wild reindeer among the Soyot, I am not moved by any fear of having my theories contradicted or my "dreams" shattered. If Olsen's observations should happen to be perfectly correct, this would not seriously impair any of my theories. I find, however, that the process of taming, described by Olsen, has a suspicious similarity to the breaking of domesticated (not wild) reindeer among the Lapp; and it seems to me very curious that the Soyot should carry on a regular domestication of wild reindeer, as long as no such procedure is known from other reindeer tribes.

On a number of points I have criticised Dr. Laufer's paper, "The reindeer and its domestication." Some of these points he passes by in silence in his reply—*e.g.*, my criticism of his note on the Lappish and Samoyed sledges. Other points he takes up for discussion. With regard to Dr. Laufer's statement that "reindeer milk is not made into any product in northern Asia," I cited two instances to the countrary; Dr. Laufer declares that does not alter his views. That may be. More serious is his maintenance of his remarkable interpretation of Othere's account. Dr. Laufer declares that he does not read more into documents than is warranted by their contents. However, he not only refuses to accept the interpretation which our best authorities so far have given of Othere's reindeer account; he actually reads into this account something which it does not contain, and which is moreover in perfect disaccord with what we know of the culture of the old Norsemen and of the habits of the reindeer of the region where Othere lived. Othere's account does not state that his reindeer were "the venture of a sportsman, who had an aesthetic pleasure in the animals, like a park-owner in fallow deer": in this interpretation, Dr. Laufer goes entirely beyond the content of our document. Furthermore, the saga-literature, which tells us a great deal about the life and culture of the old Northmen, although not much about those of other nations, does not contain anything about deer-parks (one of our best authorities on the culture of the saga-period, Professor Valtyr Gudmundsson of the University of Copenhagen, has

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verified that). And, what is also a serious objection to Laufer's interpretation, the reindeer of the region are strictly migratory. Dr. Laufer finds these two objections not valid; it seems to me—and to other students of Lappish ethnology—that they have considerable force. The refusal of Dr. Laufer to take account of them cannot be given much importance as long as he has not produced a single piece of evidence in behalf of his theory about Othere's supposed deer-park.

I am glad to learn that Dr. Laufer entertains no doubt as to the nationality of Othere's Finn. Page 95 of his paper gave me the impression that he was in doubt. Now, as Finn in Othere's narrative means Lapp, we learn from Othere that the Lapp in Othere's country in the ninth century caught wild reindeer by means of decoy deer—that is, they used tame reindeer in hunting, probably by the same hunting methods as those which were fully described by later authors on Lapland, which have been in use by reindeer nomads all over northern Eurasia. Othere does not say that he himself used his six decoy deer in hunting. He says, however, that he owned altogether a herd of six hundred tame deer. As Scandinavians are not known to have been reindeer breeders, except in a sort of cooperation with the nomadic Lapp who tended the reindeer of Scandinavian owners together with their own herds, and as Othere mentions the Lapp and their use of decoy deer at the same time as he speaks about his herd of six hundred, the inference can hardly be avoided that Othere, the Lapp, and the reindeer had something to do with each other, probably in much the same way as Scandinavian reindeer owners, nomadic Lapp, and reindeer in later centuries. If we are to understand Othere's reindeer account at all, we must read it in this way. Dr. Laufer's interpretation, on the other hand, is fanciful, as it does not agree with Othere's account, nor accord with other facts about Scandinavians and reindeer.

As I have shown (p. 125), an Icelandic poem, probably from the thirteenth century, mentions a Lapp chieftain, riding in a reindeer sledge. Dr. Laufer does not seem to have noticed that. On the other hand, he informs me of the undeniable truth, that Saxo's tale about Hotherus is legendary. I have, of course, not thought of doubting that; when I mentioned Saxo's tale, it was because an eminent folklorist has compared a passage in this tale with the reindeer-driving Lapp chieftain in the Icelandic saga and poem. As I have stated several times, we do not learn much about other nations in the sagas. I find it, however, natural to adduce what the sagas have to say on Lapp and reindeer.

Dr. Laufer's sweeping assertion, that "no historical facts should be

deduced from the status of loan-words and other linguistic phenomena" is an attack on the eminent philologists who have deduced historical and culture-historical facts from loan-words and other linguistic phenomena in Lappish and neighboring languages, and does really not concern me, as I am not a philologist nor am I claiming to be one. I have cited the opinion of recognized philologists upon Lappish reindeer nomadism, because I found it desirable to call attention to as much material as possible, bearing upon this problem. I would advise Dr. Laufer to study the particular works in question before passing his sentence. Comparative philologists have probably made many mistakes, especially in their attempts at reconstructing the original "Indogermanic" culture. I fear, however, that by condemning indiscriminately all philologic attempts at deducing culture-historical facts from linguistic phenomena, Dr. Laufer may perhaps strike at more heads than he can easily cut off.

Now we arrive at Kalevala. Unfortunately, Dr. Laufer revealed in his paper a fatal lack of understanding of the real culture-historical import of this remarkable epic. It was necessary, therefore, to state shortly what Kalevala is, according to modern folklore. It is not at all my own ideas I have set forth about Kalevala; I may, therefore, justly disclaim the honour of being ridiculed by Dr. Laufer as a self-constituted Kalevala authority. I would, however, advise Dr. Laufer to read his classics, and some modern authors too, a little more carefully, before he takes his final stand in the question of Kalevala's bearing upon reindeer nomadism.

When I maintain that the description of Lapland and the Lapps in Kalevala is not realism, I do not assert, of course, that Kalevala does not contain any glimmering of truth about the Lapps. It is, however, on the whole a distorted, fanciful, imperfect, unrealistic picture of Lappish culture that can be gained from Kalevala. Laufer cites a number of verses in which snowshoes, reindeer-hunting, and sledges drawn by horses are mentioned. I do not quite understand why he does that. Is he not aware that horse driving is a Finnish trait and never was a Lappish one, and that snowshoeing and elk and reindeer hunting are old Finnish sports?

Dr. Laufer has the audacity to assert, "Had it so happened that the Kalewala furnishes the opposite data which would support Hatt's presumptions, he would probably have accepted them without hesitation." This is glaring unfairness. If Dr. Laufer has read my paper through, he cannot have avoided noticing (p. 127) that I have cited a passage from Kalevala where the reindeer actually is mentioned as the

northern or Lappish equivalent of the horse. As I have said already in my paper, I do not quote this passage as proof of the antiquity of reindeer nomadism—which would, in my opinion, be entirely inappropriate—but solely to show that Dr. Laufer has not read Kalevala carefully enough. I confess that his reiterated assertion that Kalevala "does not contain the faintest allusion to domesticated reindeer," is evidence of a steadfast mind.

The value of a discussion depends upon the validity of the arguments which are set forth, a validity notably lacking in Dr. Laufer's recent criticism.

GUDMUND HATT.

COPENHAGEN,
October 16, 1920.

WHO WERE THE PADOUCA?

Dr. Grinnell, *American Anthropologist*, Vol. 22 (N.S.), p.248 *et seq.* discusses the question as to who the Padouca were, and states (p. 260) that "the evidence . . . convinces me that the Padouca were not the Comanche, and I am disposed to regard them as Apache." Without wishing to review his entire article, I may point out that the Foxes call the Comanche and no other people Pātō'kā'ᵃ: see William Jones, Fox Texts [1907], p. 216; and this is substantiated by my own information. It is obvious that this has an important bearing on who the Padouca were.[1]

TRUMAN MICHELSON.

BUREAU OF AMERICAN ETHNOLOGY,
WASHINGTON, D. C.

[1] Naturally Pātō'kā'ᵃ is not in the synonymy under the article Comanche, Handbook of American Indians, but Dr. Grinnell has apparently ignored the fact that other living Indian tribes also know the Comanche by equivalents of "Padouca": see the synonymy under the article Comanche in the said Handbook.

ANTHROPOLOGY AT THE PHILADELPHIA MEETING AND PROCEEDINGS OF THE AMERICAN ANTHROPOLOGICAL ASSOCIATION

The American Anthropological Association held its nineteenth annual meeting at the Houston Club, University of Pennsylvania, Philadelphia, December 27 and 28, 1920.

Three meetings of the Council were held at two of which President Wissler was in the Chair. Vice-President Swanton presided at the last meeting of the Council.

Council Meeting, December 27, 9:45 A.M.

The following reports were read and accepted:

Report of the Secretary

The proceedings of the last annual meeting of the American Anthropological Association were published in the *American Anthropologist* for January-March, 1920. There has been no special meeting of the Association nor of the Council during the year.

The Executive Committee had numerous matters brought to its attention during the year. These were as follows:

Feb. 16. In response to a circular letter of Feb. 16, the Committee voted unanimously to accept the invitation to meet in Chicago in December.

Mar. 1. The majority of the Committee voted to recommend to the National Research Council Mr. F. W. Hodge to fill out the unexpired term of Dr. Franz Boas which extended to July 1, 1920.

Apr. 17. F. W. Hodge and Clark Wissler, nominated by the Council of the A.A.A. in December, 1919, were elected by the National Research Council to serve a three year period beginning July 1, 1920.

Sept. 22. In response to a circular letter of Sept 22, the committee voted to discontinue the publication of the Memoirs. The sentiment of the Committee was against further reduction in the size of the *Anthropologist*.

Nov. 18. As the result of a petition, the Committee was asked to reconsider its decision of Feb. 16 as to the place of the next meeting.

Nov. 24. The Committee was asked to vote on Baltimore and Philadelphia as the place for the coming meeting.

Dec. 2. The Secretary was informed by the President that the majority of the Committee had voted to accept the invitation of the University of Pennsylvania and meet in Philadelphia.

The anthropological membership of the Division in the National Research Council is now as follows:

To serve until July 1, 1921: J. W. Fewkes, B. Laufer, P. E. Goddard.[1]

To serve until July 1, 1922: R. B. Dixon, A. L. Kroeber, A. M. Tozzer.[1]

To serve until July 1, 1923: Clark Wissler, F. W. Hodge, J. H. Breasted.[1]

Members of the Executive Committee: R. B. Dixon, J. W. Fewkes.

The President appointed during the year the following members to serve as a Committee on the Prehistoric Foundation in France: Charles Peabody (Chairman), G. G. MacCurdy, N. C. Nelson.

The Association has lost by death during the year four members: Professor William Churchill, Professor S. A. Lafone Quevedo, Dr. W. H. Furness, 3d., and Dr. James M. Flint.

Fifteen members have resigned, five have been dropped, and 41 new names have been added to the list of members, making a net gain of fifteen. The membership at present is as follows:

Honorary members	5
Life members	13
Regular members	499
Total	517

Respectfully submitted,

ALFRED M. TOZZER,
Secretary.

[1] Elected by National Research Council.

Report of the Treasurer

Receipts

Balance on hand, January 1, 1920			$ 319.30
Anthropological Society of Washington		$ 247.45	
American Ethnological Society		375.00	
Annual Membership Dues:			
1918	$ 27.00		
1919	174.01		
1920	1,785.90		
1921	838.28	$2,825.19	
Sale of Publications		391.61	
Rehabilitation Fund		100.00	
Reimbursements		130.40	
Interest		9.56	
Miscellaneous		1.60	$4,080.81
Total Receipts			$4,400.11

Disbursements

New Era Printing Company	$3,269.57	
Beck Engraving Company	77.27	
Treasurer-Editor's and Secretary's Expenses	470.98	
Miscellaneous	16.60	
Total Disbursements	3,834.42	
Cash on hand	565.69	
	$4,400.11	$4,400.11

Resources

Cash on hand, December 20, 1920			$ 565.69
Due from engravings		$ 6.84	
Due from sales:			
1919	$ 5.50		
1920	151.72	157.22	
Due from dues:			
1918	$ 24.00		
1919	48.00		
1920	182.00	254.00	418.06
Total Resources			$ 983.75

Liabilities

Membership dues from 1921 already paid	$ 838.28	
Total Liabilities	837.28	
Net excess of Resources over Liabilities	145.47	
	$ 983.75	$ 983.75

Cost of Publications

American Anthropologist, vol. 21, no. 4:			
Engravings	$ 6.70		
Printing	461.24	$ 467.94	$ 467.94.
American Anthropologist, vol. 22, no. 1:			
Engravings	$ 3.68		
Printing	468.46	$ 472.14	
Reinbursements		3.68	$ 468.46
American Anthropologist, vol. 22, no. 2:			
Engravings	$ 25.87		
Printing	390.42	$ 416.29	
Reimbursements		19.03	$ 397.26
American Anthropologist, vol. 22, no. 3:			
Engravings	$ 21.13		
Printing	464.94	$ 486.07	
Reimbursements		21.13	$ 464.94
Net Cost			$1,798.60
Memoirs, vol. 6, no. 3:			
Printing	$ 398.09	$ 398.09	
Reimbursements		71.56	$ 326.53
Memoirs, vol. 6, no. 4:			
Printing	$ 289.65	$ 289.65	$ 289.65
Net Cost			$ 616.18
American Anthropologist, net cost		$1,798.60	
Memoirs, net cost		616.18	$2,414.78
Reprints and Distribution (*American Anthropologist* and *Memoirs*).			242.04
Total Cost			$2,656.82

PERMANENT FUND

Receipts

Balance, January 1, 1920		$1,313.62
Interest, May 17, 1920	$ 4.24	
Interest, December 7, 1920	4.26	8.50
Total Receipts		$1,322.12

Investments

Liberty Bonds, June 11, 1919	$ 190.90	
W. S. S., June 11, 1919	16.68	
W. S. S., October 21, 1919	4.21	
W. S. S., May 17, 1920	4.16	
W. S. S., December 13, 1920	4.23	$ 220.18
Cash in envelope		1.94
Loan to General Fund		1,100.00
Total Investments		$1,322.12

The accounts of the Treasurer, P. E. Goddard, have been examined and found correct.

M. H. SAVILLE,
R. H. LOWIE,
Auditing Committee.

The income of the Association has increased slightly this year over the preceding years. At the last annual meeting the charge for publications furnished to the Ethnological Society and the Washington Society was increased from $3.50 to $4.00 per volume. The total amount of the dues for 1920 is $2,207.70 of which $339.88 was received in 1919 and $182.00 remains to be collected. The total for 1919 is $2,200.88 of which $48 is still uncollected. The sales have been unusually large, $543.33 as against $211.27 for 1919. This is a result of an increasing demand by libraries for a complete set of the *American Anthropologist* to meet the requirements of their patrons, which it is hoped will be permanent, and in part to a recovery from the war, which is temporary. The sum of $100.00 has been added to the Rehabilitation Fund by spontaneous donations.

In May, word was received from our printers that there would be a further increase of about 25 per cent. in the manufacturing expense. By reducing the number of pages from 500 to 400 it has been possible to keep within the budget of $2,000.00 for the *American Anthropologist*. The amount paid for printing four numbers is $1,798.60. The reprints and postage bill of the New Era Printing Co. covering both the two numbers of the *Memoirs* and the four numbers of the *Anthropologist* is $242.04 which when added to the cost of printing amounts to $2040.64.

The allowance for office expenses was $500 of which $470.98 has been used.

There has been sufficient income to pay for the two numbers of the *Memoirs* for 1919 amounting to $616.18.

For the first time in years our bills are all paid and we have a balance in the bank of $565.69. The statement of resources and liabilities shows an improvement in our condition of $150.82.

Respectfully submitted,

P. E. GODDARD,
Treasurer.

REPORT OF THE EDITOR

Owing to an advance in the cost of printing, the *American Anthropologist* was reduced from the intended 500 pages to 400. The volume con-

tains articles of both general and special interest. As examples of the former may be mentioned the address by President Wissler on "Opportunities for Coordination in Anthropological and Psychological Research" and one by Prof. Boas entitled "The Methods of Ethnology." Such articles mark for ourselves, and others who may be interested, the progress made in recent anthropological thinking. Among the articles of less general interest should be noted one by Dr. Gamio printed in Spanish on "Las Excavaciones del Pedregal de San Angel y la Cultura Arcaica del Valle de Mexico."

The review section has been conducted by Dr. Lowie, Associate Editor. These reviews, as in past years, serve as a ready source of information as to current anthropological writings and as a convenient means for comment on the methods and principles involved. In the opinion of the Editor, the department of Discussion and Correspondence also furnishes a much needed opportunity for a free expression of opinion in regard to newly discovered facts, current theories, and general criticism of recent publications.

There is manifest a growing interest in anthropology, both on the part of educational institutions and the reading public. An increase in the circulation of the journal amounting to 25 or 50 per cent. would produce revenue sufficient for comfortable and ample publication.

Respectfully submitted,

P. E. GODDARD,
Editor.

The Treasurer moved the following recommendation for the budget for 1921, which was adopted:

For manufacturing and distributing the American Anthropologist and authors' reprints	$2,500.00
For providing engravings in special cases	150.00
For the expenses of the Editor, Treasurer, and Secretary	550.00
Total	$3,200.00

The resignation of the Secretary was accepted with regret and a vote of thanks was passed.

Thirty-seven members were elected. Their names are as follows:

Julius Springer
W. Leon Godshall
E. R. Groves
C. Harris
Dr. J. H. Kellogg
Prof. Campbell Bonner
G. E. Nitzsche
P. J. Patterson
Prof. E. L. Patton
Dr. E. B. Renaud

Margaret J. McCoy
F. C. Meredith
Otto Müller
New School for Social Research
R. F. Warren
Dr. Spencer Trotter
W. H. Over
W. J. Wintemberg
Dr. Aristides Mestre
Union Medical College Library, Pekin
P. F. Scott
Sociedad cientifica "Antonio Alzate"
Dr. N. Utsurikawa
P. Sherwin
Sir Nil Ratan Sircar
Tioga Point Museum
U. S. Public Health Service
University of Denver
Whitcombe and Lombs
University of Bristol
University of Otago
D. Jenness
Prof. C. S. Brown
Minna B. Fensin
L. S. Shotridge
C. E. Story
R. J. Weitlauer
Frank Wood

It was moved and passed that the communication of Arthur MacDonald be laid upon the table.

It was moved and passed that a set of the *American Anthropologist* be presented in the name of the Association to the Library of the University of Louvain.

The following reports were read and accepted:

Report of the Chairman of the Joint Committee of the American Anthropological Association and of the Archaeological Institute of America, December, 1920

The enclosed report of the meeting of the Joint Committee in New York on April 22nd, 1920, prepared by the Secretary of the Committee, carries the history of the proposed American Foundation in France for Prehistoric Studies up to that time.

The Chairman of that Committee has the honor to report as follows:

The first year's budget of 21000 francs is guaranteed.

It is hoped that a few more subscriptions or guarantees may come in so that the individual subscriptions or guarantees need not be received or demanded in full; while the Foundation may never be self-supporting, it is a sign of financial health when a less sum is called for than is legally due.

A list of subscriptions and guarantees is appended to this report.

The Chairman is grateful to the individuals on that list and to others who have helped. Raising the money has not been an ungrateful task.

In July the Chairman went abroad and returned in December, 1920;

much of that time was spent in securing the good will of our French colleagues, without which the institution can not live, and without the securing of which the Chairman was unwilling to continue efforts in behalf of it.

Most of the leading anthropologists of France were made acquainted with the project, through being presented with an identical "Projet de Fondation."

No opposition was encountered.

Some suggestions were received, which the Director may, if he chooses, accept or modify.

In particular the cordial sympathy of the École d'Anthropologie de Paris, the Institut de Paléontologie Humaine, the Museum d'Histoire Naturelle and the great National Museum at Saint-Germain seems assured.

During an interview secured just before returning the Chairman was assured by the Director des Beaux Arts (under whose authority the excavations of the Foundation will come) that he was "d'accord" with our plans; a formal request for "Reconnaissance"—not to be confounded with "Reconnaissance d'Utilité Publique"—has been sent by Ambassador Wallace, with a cordial approbation of his own through the intermediary of M. George Leygues, President of the Council, to M. Paul Leon, Directeur des Beaux Arts; this will require possibly several months and a written answer; it is by no means necessary to wait for this before beginning work.

There are no legal interdictions on our plans; there are two laws, one of some years' standing and one passed a few months ago, which will place our exportation of specimens under a more or less official control; with tact, this will cause the Director no trouble.

The Chairman begs, therefore, that in accordance with the vote of April 22d, 1920, the Council will, at its earliest opportunity, appoint the three members of the Governing Board, and that it will recognize officially the existence of the American Foundation in France for Prehistoric Studies.

CHARLES PEABODY,
Chairman.

REPORT ON THE MEETING OF THE JOINT COMMITTEE

The Joint Committee on the proposed American Foundation in France for Prehistoric Studies met at the Hotel Plaza on April 22d, 1920.

Present: C. Peabody (Chairman), C. Wissler, G. H. Chase, N. C. Nelson, G. G. MacCurdy.

The Chairman explained the reasons for the calling of the Committee also stating that Dr. Henri Martin's Laboratory and the principal part of the station at La Quina are now the property of the French Government.

The Committee unanimously agreed that in order to insure the permanence of the Foundation the interest and aid of the French Government should be secured in addition to any promise of, or agreement with, Dr. Henri Martin; and that in the light of the records of other existing American schools of research in foreign lands, it would be highly desirable to found the one proposed in the field of prehistoric research.

It was voted to add to paragraph 4 of the chairman's synopsis the following: "Said specimens of human skeletal remains discovered by said Foundation and left in Charente, shall always be accessible to American students whether members of the Foundation or not."

The Committee recommends that a Governing Board of six be appointed by the Councils of the two constituent societies, three by each, and that this Board be empowered to appoint members at large not to exceed three.

This Governing Board shall appoint a Director of the Foundation annually. At the suggestion of the Chairman, the Committee voted that one of its number, Professor G. G. MacCurdy, be asked to serve as first Director for the proposed Foundation. It also moved that any members of the Committee who may be abroad this summer shall constitute a subcommittee with power to act.

It recommends that a Foundation Scholarship of 2000 francs be added to the proposed budget.

An affirmative vote was taken on the following recommendations:

1. That a letter of approval be secured from Dr. J. Walter Fewkes, Chief of the Bureau of American Ethnology.

2. That the cooperation be secured of M. J. J. Champenois, New York representative of the Office National Universités et des Écoles Françaises, and of Stephen P. Duggan, Chairman of the Committee on Fostering International Relations.

G. G. MacCurdy,
Secretary.

The President appointed the following nominating committee: R. B. Dixon, W. Hough, and R. H. Lowie.

The Council adjourned at 10:30 A.M.

Council Meeting, December 27, 5 P.M.

The reports of the committees on the Cahokia Mound and on legislation in Maine were read and accepted. It was moved and passed that these two committees be discharged.

The Chairman of the Committee on the Cambridge meeting presented the following resolution which was passed:

Resolved: That on behalf of the members of the Association in attendance at the annual meeting of 1919, we express our appreciation of the sincere welcome extended to us by the officers and members of the Council residing in and around Boston, to whom, individually and collectively, we are indebted for many personal hospitalities, and through whose efficient cooperation we were privileged to have a most stimulating session.

Report of the Publication Committee

The following report of the Publication Committee was read and accepted after various amendments had been voted down:

The Publication Committee recommends to the Council that in the future each Publication Committee be directed to elect from its members, but not including the Editor or Associate Editors, a subcommittee of three which shall serve as an advisory body for the Editor, which shall decide any differences which may arise between authors and the Editor and with the Editor shall be charged with the conduct of the publications of the Association.

It is recommended that the Editor be empowered to print extra copies of the *American Anthropologist* not to exceed 100 which copies shall be supplied to members in Europe and Latin American countries at the rate of exchange existing in 1914 before the war provided that the amount is not less than the additional cost of printing such copies.

It is recommended that the Editor be given power to issue Memoirs from time to time as funds may be available but that such Memoirs shall bear serial numbers without a volume number and that each Memoir be issued complete in itself with an index.

Three new members of the Association were elected. Their names are as follows: E. R. F. Johnson, M. M. Dorizas, H. Z. Heronimakis.

After changes made at this meeting and at a subsequent meeting of the Council held on Tuesday, December 28, at 9 a.m. the following list of officers was offered by the Nominating Committee:

President: W. C. Farabee.

Vice-President: 1924, G. G. MacCurdy.

Secretary: A. V. Kidder.

Treasurer-Editor: J. R. Swanton.

Executive Committee: F. W. Hodge, G. B. Gordon, J. W. Fewkes, C. Peabody.

Council: (1924) Byron Cummings, G. G. Heye, H. J. Spinden,

C. M. Barbeau, W. D. Wallis, A. B. Lewis, S. Hagar, H. N. Wardle, S. K. Lothrop, R. T. Aitken, J. E. Pearce, F. Starr, W. E. Gates, M. R. Harrington, S. J. Guernsey, W. W. Hyde, B. F. Schappelle, W. E. Meyer, S. Trotter, E. P. Wilkins, W. F. Ogburn, E. S. Handy, C. L. Hay, J. B. Stetson.

Representatives of the Association on the National Research Council to serve for three years from July 1, 1921: B. Laufer and J. W. Fewkes.

Delegates for the Association to Section H of the A.A.A.S.: C. Wissler and J. W. Fewkes.

Committee on the Prehistoric Foundation in France: C. Peabody, G. G. MacCurdy, and N. Nelson.[1]

The following resolution was proposed and passed:

Resolved: That the Association tender to Dr. Goddard their sincere appreciation of his services as Editor and of his most successful management of the funds of the Association as Treasurer. The Association would also like to express its satisfaction with the policy of Dr. Goddard in the development of the *Anthropologist*, in stimulating the departments of reviews and discussion, and in the securing of foreign articles.

The Council adjourned at 6 P.M.

Council Meeting, Tuesday 9 A.M.

It was resolved by motions duly made and passed:

That the Executive Committee be given power to choose the place of the next meeting of the Association.

That a vote of thanks be given to the retiring President.

That a vote of thanks be given the authorities of the University Museum of the University of Pennsylvania.

That a vote of thanks be given to the Provost and other authorities of the University of Pennsylvania for its abundant hospitality.

That a vote of thanks be given Dr. Speck for his activities in behalf of these meetings.

That members elected at a meeting and not having paid their dues for the year in which they are elected shall not vote at that meeting.

The Council adjourned at 10 A.M.

Annual Meeting, December 28, 10:30 A.M.

The names of A. M. Tozzer, P. E. Goddard, H. N. Hall, and Theresa Mayer were added to the list of members nominated for the Council.

[1] The three other members of this Committee, elected by the Council of the Archaeological Institute of America, are G. H. Chase, W. N. Bates, and W. N. Stearns.

The officers and members of the Council as nominated were duly declared elected by a vote ordered cast by the Secretary.

The President, Dr. William C. Farabee, appointed the following committees:

Committee on Program: F. G. Speck (chairman), A. V. Kidder (sec'y *ex-officio*), P. E. Goddard, H. N. Hall, M. H. Saville, C. M. Barbeau, S. J. Guernsey, N. M. Judd, S. K. Lothrop.

Committee on Finance: R. B. Dixon (chairman), C. P. Bowditch, W. E. Gates, G. G. Heye, C. B. Moore, E. E. Ayer, W. H. Furness, C. L. Hay, A. M. Huntingdon, J. B. Stetson, Jr.

Committee on Policy: Clark Wissler (chairman), W. H. Holmes, S. Culin, G. B. Gordon, W. C. Mills, C. Peabody, F. Boas, J. W. Fewkes, A. E. Jenks, E. C. Parsons, A. M. Tozzer.

Committee on Publication: W. C. Farabee (chairman, *ex-officio*), J. R. Swanton (sec'y *ex-officio*), S. A. Barrett, S. Culin, J. W. Fewkes, A. A. Goldenweiser, F. W. Hodge, E. A. Hooten, A. Hrdlička, A. E. Jenks, A. L. Kroeber, B. Laufer, G. G. MacCurdy, W. K. Moorehead, S. G. Morley, E. Sapir, H. J. Spinden, H. N. Wardle, C. Wissler.

The following advisory subcommittee was subsequently elected by the Committee on Publication in accordance with the vote of the Council as given above (page 111): F. W. Hodge (chairman), J. W. Fewkes, B. Laufer.

The following papers were presented:

J. H. PENNIMAN, *Acting Provost;* Address of Welcome.

A. I. HALLOWELL, More alien influences in America.

CHARLES PEABODY, Report on the Prehistoric Foundation in France.

P. E. GODDARD, Notes on the Wailaki of California.

FRANZ BOAS, The methods of ethnological research.

T. MICHELSON, Some notes on the Plains Cree.

W. C. FARABEE, Prehistoric South American gold models of throwing-sticks.

H. N. WARDLE, A double-bowled pipe, probably Peruvian.

H. S. COLTON, The petroglyphs in Picture Canyon.

A. V. KIDDER and S. J. GUERNSEY, Early cultures in northeastern Arizona.

S. K. LOTHROP, The Nahua tribes of Nicaragua, Costa Rica, and Panama.

W. K. MOOREHEAD, Ruins in the Panhandle of Texas.

J. R. SWANTON, The social organization of the southeastern Indians.

A. A. GOLDENWEISER, Some contributions to totemic theory.

ELSIE CLEWS PARSONS, Kinship nomenclature of the Pueblo Indians.

R. H. LOWIE, Cultural relations of the Plateau Shoshoneans and the California Indians.

ERNA GUNTHER, Designs of Tlingit basketry (with remarks by L. Shotridge).

The following papers were read by title:

J. W. FEWKES, The Fire Temple, Mesa Verde National Park.

GRACE DANGBERG, Social life of the Washo.

H. N. HALL, Some hunting methods of the Yenisei Samoyed and Yurak.

Papers relating to Folk-Lore were presented by Elsie Clews Parsons, F. G. Speck, Theresa Mayer, and Gladys Reichard at the meeting of the American Folk-Lore Society. Folk-Lore papers by Georgiana G. King and P. E. Goddard were read by title.

The members enjoyed a dinner at the T-Square Club on Monday, December 27, and they were the guests of the Maya Society at a dinner on Tuesday, December 28.

ALFRED M. TOZZER,
Secretary.

ANTHROPOLOGICAL NOTES

Obituary—Professor H. P. Steensby

Anthropologists on this continent will learn with regret of the death, on October 12, 1920, of Professor H. P. Steensby, one of Denmark's foremost geographers and students of Eskimo culture. Like so many of his fellow-countrymen Professor Steensby was early interested in Greenland, and gained his Ph.D. degree at the University of Copenhagen with a treatise "On the Origin of Eskimo Culture." For a time he subordinated ethnology to geography, and in 1908 travelled in Algeria and Tunisia; but a visit to West Greenland in 1909 revived his interest, and from the year 1911, when he became Professor of Geography in the University of Copenhagen, he devoted all his energies to the elucidation of the geographical, historical, and ethnological problems of that country. He was a notable contributor to the Middelelser om Grønland, publishing several treatises that have gained wide-spread attention. His best known work is his "Anthropological Study of the Origin of Eskimo Culture," in volume 53 of that journal, where he tried to prove, by an analysis of the geographical conditions surrounding each branch of the Eskimo race and of the main culture-elements, that the original home of what is distinctively Eskimo must be placed in the region of Coronation Gulf. This was the cradle of the Eskimo race, he believed, but when, through the exhaustion of the musk-oxen, apparently, it was compelled to spread out, east and west and north and south, in search of new food areas, the culture of the different bands became more and more diverse, partly through the influence of their new surroundings, partly through contact with neighboring peoples. After this work was published Professor Steensby took up the problem of the Norse discovery of America, and thought to locate on the shores of the Gulf of St. Lawrence the site of the original Vinland. He published a treatise on this topic in 1917, but in order to familiarize himself more with the geography of the region, he came over to Canada last summer. It was on his voyage back to Denmark that he suddenly fell ill and died.

American Foundation in France for Prehistoric Studies

Among the Proceedings of the American Anthropological Association at its last meeting, printed in this number of the *Anthropologist*, will be

found the Report of the Chairman of the Joint Committee of the American Anthropological Association and the Archaeological Institute of America upon the project for the establishment of an American Foundation in France for Prehistoric Studies. This report, which not only assured the two societies of the desirability and feasibility of the plan but informed them that the budget for the first year had been guaranteed, was accepted, and a Governing Board of six was appointed.

On February 3, 1921, a meeting of the Governing Board was held at the Hotel Plaza, New York, and Prof. George Grant MacCurdy was elected first Director of the Foundation. Dr. Charles Peabody is Chairman of the Board and for the present will also serve as Treasurer of the Foundation.

The year's work will open at La Quina (Charente) on July 1st. After a stay of some three months at La Quina, there will be excursions in the Dordogne, in the French Pyrenees, and to the Grimaldi caves near Mentone. The winter term will be in Paris, and the work of the spring term will include excursions to the important Chellean and Acheulian stations of the Somme valley, to Neolithic sites of the Marne or some other suitable locality, and to Brittany for the study of megalithic monuments.

Students may enroll for an entire year or for any part thereof. Those who contemplate entering, either for the year or for the first term, should communicate immediately with the Director, at Yale University Museum, New Haven, Conn., or with Dr. Charles Peabody, Peabody Museum, Cambridge, Mass.

One Foundation scholarship of the value of 2,000 francs is available for the first year. The special qualifications of the applicant, together with references, should accompany each application. The Foundation is open to both male and female students.

The address of the Director after June 15th will be "care of Guaranty Trust Company, Paris."

THE SEVENTIETH ANNIVERSARY OF DR. J. WALTER FEWKES, Chief of the Bureau of American Ethnology, which occurred on Nov. 14, 1920, was celebrated at a luncheon in the Smithsonian building, Washington, D. C., attended by about forty of his friends and associates. On this occasion he was presented with a specially bound volume of letters of congratulation contributed by his acquaintances in all parts of the country. Dr. Fewkes has recently been elected an Honorary Member of the Société des Américanistes de Paris.

DR. FRANZ BOAS, Professor of Anthropology in Columbia University, has been elected an Honorary Member of the Société des Américanistes de Paris and of the Folklore Society of London. He has also been elected a Corresponding Member of the Prussian Academy of Sciences and has received the gold medal of the Anthropological Society of Berlin.

MR. PHILIP AINSWORTH MEANS was appointed in May of last year to the directorship of the Museo Nacional de Arquelogía in Lima, Peru, assuming office in November.

EARLY in November Dr. J. W. Fewkes accompanied Mr. Stephen T. Mather, Director of the National Park Service, and several gentlemen connected with the Denver & Rio Grande Railroad, on a trip of inspection to the Mesa Verde National Park, Colorado.

THE degree of Doctor, *honoris caûsae*, has been conferred upon Dr. Aleš Hrdlička, Curator of Physical Anthropology in the U. S. National Museum, by Prague University.

ON Nov. 8, Dr. Arístides Mestro was made Honorary Professor of Anthropology in the Museo Anthropologico Montané, University of Habana.

DR. LUIS MARIA TORRES, Head Professor of the Archaeological and Ethnographical Departments and Professor of American Prehistory in the National University of Argentina, has been appointed Director of the La Plata Museum.

PROF. CARL TOLDT, President of the Anthropological Society of Vienna, died on November 13 last. A memorial meeting in his honor was held at the University of Vienna on December 13.

MR. J. A. JEANÇON, who had been filling an appointment as Special Archaeologist in the Bureau of American Ethnology, has accepted the position of Director and Curator of Archaeology in The State Historical and Natural History Society of Colorado.

MR. SYLVANUS G. MORLEY of the Carnegie Institution, and Mr. William E. Gates are engaged in researches among the Mayan peoples of Guatemala.

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